VAN NOSTRAND'S

ECLECTIC

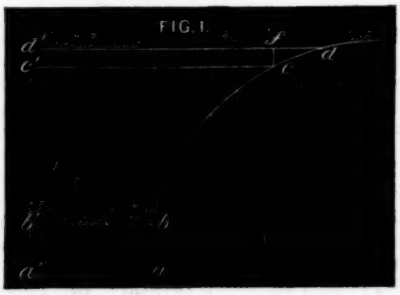
ENGINEERING MAGAZINE.

NO. LXXXVII.—MARCH, 1876.—VOL. XIV.

THE DIFFERENCE OF THERMAL ENERGY TRANSMITTED TO THE EARTH BY RADIATION FROM DIFFERENT PARTS OF THE SOLAR SURFACE.

By Capt. J. Ericsson.

Previous to undertaking a systematic investigation of the above subject, I examined thoroughly the merits of Laplace's famous demonstration relating to the absorptive power of the sun's atmosphere, proving that only $\frac{1}{17}$ of the energy developed by the sun is transmitted to the earth. The demonstration being based on the assumption that the sun's rays emit energy of equal intensity in all directions, my initiatory step was that of testing practically the truth of that proposition. It has been asserted that Laplace did not propound the singular doctrine involved in such a proposition, I therefore feel called upon, before proving its unsoundness, to quote the words employed by the celebrated mathematician (see *Méchanique Céleste*, Tom IV., page 284). Having called attention to the fact that any portion of the solar disc as it approaches the limb, ought to appear *more brilliant*, because it is viewed under a *less angle*. Laplace adds: "Car il est naturel de penser que chaque point de la surface du soleil renvoie une lumière égale dans tous les sens." Let *a b c d* in the annexed diagram, Fig. 1,



represent part of the border of the sun, and *b a*, *c d*, small equal arcs; *a a'*, *b b'*, *c c'*, *d d'*, being parallel rays projected towards the earth. Laplace's theory

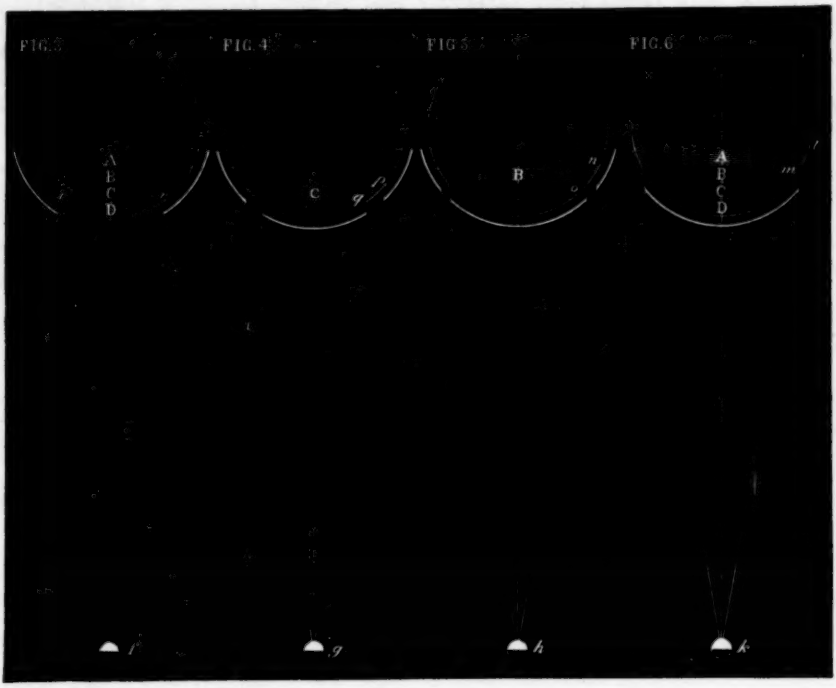
asserts that owing to the concentration of the rays, the radiation emanating from the portion *d c* transmits *greater* intensity towards the earth than *b a*, in the proportion of *c d* to *f c*. The proposition is thus stated *in Mechanique Céleste*: "Call θ the arc of a great circle of the sun's surface, included between the luminous point and the centre of the sun's disc, the sun's radius being taken for unity; a very small portion *a* of the surface being removed to the distance θ from the centre of the disc, will appear to be reduced to the space *a* cos. θ; the intensity of its light must therefore be increased, in the ratio of unity to cos. θ."

In order to disprove the correctness of the stated demonstration, I have measured the relative thermal energy of rays projected in different directions from an incandescent metallic disc, by the following method:

Figure 2 (see plate) represents section of a conical vessel covered by a movable semi-spherical top, the vessel being surrounded by a jacket through which water may be circulated. A revolving circular disc *a a*, composed of cast iron, the back being semi-spherical and protected by fire-clay, is suspended across the top of the conical vessel, supported by horizontal journals attached at opposite sides. The angular position of the disc is regulated by a radial handle *b* connected to one of the journals, the exact inclination to the vertical line being ascertained by means of a graduated quadrant *d*. An instrument *c* capable of indicating the intensity of the radiant heat transmitted by the incandescent disc, is applied at the bottom of the conical vessel. The mode of conducting the experiment is extremely simple. The movable cover and its lining of fire-clay having been removed, the cast iron disc is heated in an air-furnace to a temperature of 1,800° F. It is then removed by appropriate tongs, and suspended over the conical vessel, the lining and cover being quickly replaced. The temperature shown by the instrument at the bottom of the conical vessel resulting from the action of the radiant heat of the disc, is then recorded for every tenth degree of inclination. The investigation, it may be briefly stated, shows that the temperatures imparted by radiation to the recording instrument, is exactly as the sines of the angles of inclination of the disc. Hence, at an inclination of 10 deg. to the vertical line, the temperature imparted to the thermometer is scarcely ⅛ of that imparted when the disc faces the thermometer at right angles; yet in both cases an *equal amount of surface of an equal degree of incandescence*, is radiating towards the instrument! Laplace, and his followers, have evidently overlooked this important and somewhat anomalous fact, proving that radiation emanating from heated bodies is incapable of exerting full energy in more than one direction. Our practical experiments with the revolving incandescent disc, have thus fully demonstrated the truth of the proposition intended to be established, namely, that the rays emanating from incandescent planes do not transmit heat of equal intensity in all directions, the energy transmitted being as stated, proportionate to the sines of their angle of inclination to the radiating surface.

The next step in the investigation of solar heat, before adverted to, was that of measuring the radiant energy transmitted in a given direction by an incandescent solid metallic sphere. For this purpose I employed a double conical vessel, similar to the one represented in Fig. 2, the incandescent sphere being suspended over the conical vessel, in the same manner as the revolving disc. The nature of the arrangement will be readily understood by inspecting the annexed diagram, which represents four spheres, Figs. 3, 4, 5 and 6; each sphere being divided into four zones, A, B, C and D, occupying unequal arcs, but containing equal convex areas. Semi-spherical screens, composed of non-conducting substances, were applied below each sphere, provided with annular openings arranged, as shown in the diagram. Through these annular openings, the radiant heat from the incandescent zones D, C, B and A was transmitted to the thermometers *f*, *g*, *h* and *k* respectively. Père Secchi, and other followers of Laplace, will be surprised to learn that when the suspended sphere was maintained at a temperature of 1,800° F., the radiation from the zone C, Fig. 4, imparted a temperature of 27°.49 F. to the thermometer *g*, while the radiation from the zone A,

Fig. 6, imparted only 6°.19 F. to the thermometer *k*. Let us bear in mind that the radiating surface *l m* of the zone A is equal to the radiating surface *p q* of the zone C. The stated great difference of temperature, produced by the radiation from zones of equal area, furnishes additional proof that Laplace based his remarkable analysis on false premises. "The sun's disc ought to appear more brilliant towards the border because viewed under a less angle," we are told by the great analyst. The instituted practical tests, however, prove positively



that the energy of the rays projected from the border of an incandescent sphere, is greatly diminished *because viewed under a less angle* from the point occupied by the recording thermometer.

The result of our experiment with the revolving incandescent disc, shows that if the small arc *b a*, in Fig. 1, be reduced until the field represented by *b' a'* becomes equal to the field represented by *c' d'*, the radiant energy transmitted through each of those fields will be alike; the reason being that the number of rays of diminished intensity passing through *c' d'* will be as much greater than the number of rays of maximum intensity passing through *b' a'*, as *c d* is greater when the reduced *b a* = *f c*. It should be observed that *c d* is so small that we may without appreciable error regard it as a straight base, and *f c* as the sine of the angle *c d f*. It follows from this demonstration that if the solar atmosphere exerted no retarding influence, the radiant heat transmitted towards the earth would be alike for equal areas of the solar diso—more correctly, for areas subtending equal angles, since the receding part of the solar surface is at a greater distance from the earth than the central part.

Encouraged by the practical result of the instituted investigation, I devised the method before described, proving positively that the polar and equatorial regions of the solar disc transmit radiant

heat of equal intensity to the earth, and that the sun emits heat of equal energy in all directions. Adopting Secchi's doctrine relating to the retardation suffered by calorific rays in passing through atmospheres, viz., that the diminution of energy is as the depth penetrated by the rays, we have shown by an easy calculation, based on the result of our investigation, that the absorption by the solar atmosphere cannot exceed ¼ of the radiant energy emanating from the photosphere.

Concerning the plan resorted to by the Director of the Roman Observatory, and others, of investigating the sun's image, instead of adopting the method of *direct* observations, I will merely observe, in addition to what has already been stated, that the information contained in the several works of the Roman Astronomer, furnishes the best possible guide in judging of the efficacy of *image investigation*. Let us select his account of the investigations conducted between the 19th and 23d of March, 1852. Having pointed out that in these experiments it was impossible to approach within a minute of the edge of the sun, and that during a later observation—date not mentioned—he had approached within a minute, the investigator observes: "But at this extreme limit, even making use of the most accurate means of observation, we find difficulties which it is impossible to overcome completely." In addition to this emphatic expression regarding the difficulties encountered, the author adds: "Moreover, it is impossible to study the edge alone, for the unavoidable motions of the image do not admit of its being retained at exactly the same point of the pile; we have therefore been unable to push the exactness as far as we hoped; and we have discontinued the pursuit of these researches, although the results obtained are quite interesting." (See revised edition of Le Soleil, vol. I., page 205.) It is needless to institute a comparison between a system of which its founder speaks so despondingly, and one which enables us to push our investigation to the extreme limit of the solar disc, admitting of entire zones being viewed at once, instead of only small isolated spots.

ON WHITWORTH'S PLANES, STANDARD MEASURES, AND GUNS.

By Prof. TYNDALL, D. C. L., LL. D., F. R. S., &c.

Proceedings of the Royal Institution.

Born on the 21st. December, 1803, Sir Joseph Whitworth, to whom we are indebted for the materials of this evening's discourse, was placed at the age of fourteen under the care of his uncle, a millowner in Derbyshire. He quitted the mill at eighteen; went to Manchester, and worked there for four years, under Crichton, Marsden and Walker, and other employers. At the age of twenty-two he went to London, lived their for eight years as a journeyman mechanic, working in succession in the establishments of Maudslay, Holtzapfel, Wright, and Clements. In 1833 he returned to Manchester, rented a room with steam power, and wrote over his door "Joseph Whitworth, tool-maker from London." The ground on which his works now stand in Manchester is worth nearly a quarter of a million sterling.

His aim from the first was mechanical veracity, and his earliest step towards the realization of this aim was the production of true plane surfaces. The most accurate planes, when he begun, were obtained by first planing and then grinding the surfaces. They were never true. He abandoned grinding for scraping. Taking two surfaces, as accurate as the planing tool could make them, he thinly coated one of them with coloring matter, and rubbed the other over it. Were both surfaces true, the coloring matter would spread itself uniformly over the upper one. It never did so, but appeared in spots and patches. These

marked the eminences, which, with an appropriate tool, he scraped away. In this way he gradually rendered the surfaces more and more coincident. But the coincidence of true surfaces would not prove them to be planes. If one were concave and the other convex, they might still coincide. This was got over by taking a third surface, and adjusting it to both the others. Were one of the latter concave and the other convex, the third plane could not coincide with both. By a series of interactions and adjustments all three surfaces were at length rendered coincident; then, and not before, were they considered "true planes."

When one true plane is placed upon another, the former *floats* upon the latter, as if some lubricating material existed between them. But when one of them is slidden, with pressure, over the other, they cling firmly together. The effect is thus described and accounted for by Sir Joseph Whitworth: "If one of them [the planes] be carefully slided over the other to exclude the air, the two plates will adhere together with considerable force, by the pressure of the atmosphere." This clinging together of flat surfaces had been noticed before the time of Robert Boyle, and that great experimenter, and noble philosopher, first gave currency to the explanation adopted by Sir Joseph Whitworth. He experimented with slabs of marble, and with plates of glass, and thought he had proved that the clinging together did not occur in vacuo. This explanation was experimentally tested in the discourse. Two exceedingly accurate hexagonal planes remained adherent in the best vacuum obtainable by a good air-pump. The vacuum was still further improved by filling the receiver with carbonic acid, and absorbing the residue with caustic potash. In this way the atmosphere was reduced until its total pressure on the surface of the hexagon amounted to only half a pound. The lower plate weighed 3 lbs., and to it was attached a mass of lead weighing 12 lbs. Though the pull of gravity was here thirty times the pressure of the atmosphere, the weight was supported. Indeed, it was obvious, when an attempt was made to pull the plates asunder, that had a weight of 100 lbs., instead of 12 lbs., been attached to the lower hexagon, it also would have been sustained by the powerful attraction of the two surfaces.

To show the probable character of the contact between the planes, two very perfect surfaces of glass were squeezed together with sliding pressure. They clung, apparently as firmly as the Whitworth planes. Throwing by reflexion from the glass plates a strong beam of light upon a white screen, the colors of "thin plates" were vividly revealed. Clasping the plates of glass by callipers, and squeezing them, the colors passed through various changes. When monochromatic light was employed the successions of light and darkness were numerous and varied, producing patterns of great beauty. All this proves that, though in such close mechanical contact, the plates were by no means in optical contact, being separated by distances capable of embracing several wave-lengths of the monochromatic light.

Having obtained his true planes, Whitworth turned them to account in producing standard measures. His paper on this subject is of profound practical interest. His aim is to spread abroad notions of attainable mechanical accuracy, far in advance of his time. He pitted the sense of touch against the sense of sight,—end-measurement, felt by the fingers, against line-measurement seen by the eye,—and succeeded in proving that the millionth part of an inch was capable of accurate measurement. With instruments capable of this accuracy he produced his standard screws and "different gauges," urging impressively upon the mechanicians of his age the enormous waste which might be avoided by the adoption of uniform dimensions, capable, when accurately measured, of being accurately reproduced.

The mechanical genius of Mr. Whitworth was next applied to the improvement of the rifle. But a few preliminary words on the use of rifling may be introduced here. When a lead bullet is cut in two, a cavity is always found within, produced by the contraction of the metal. This, and other causes, render coincidence between the centre of gravity and the geometric centre of the ball the exception, and not the rule; and where this coincidence does not exist the bullet

is almost sure to quit the gun with a motion of rotation added to its motion of translation. It is then found to deviate, sometimes to the right, sometimes to the left of the true trajectory. Sometimes, moreover, it exceeds, and sometimes falls short of the proper range. The cause of this deviation was first assigned by a philosopher who made the world his debtor in two ways: firstly, by his own important original contributions to science; and secondly, by the liberal aid and encouragement which he was always ready to extend to the younger scientific worker. I allude to the late Professor Magnus of Berlin.

Magnus's explanation of the deviation of projectiles is by no means difficult to grasp. That the deviation was produced by no shock of the projectile against the muzzle of the gun was proved by the fact that the deviation augmented in a quicker ratio than the distance from the gun. The cause of the deviation must be sought in the external air. If we blow through a tube so as to cause a current of air to pass close to the flame of a candle, the flame, instead of retreating, bends towards the current. The atmospheric pressure, in fact, is less in the current than elsewhere; hence arises a motion of the adjacent air towards the current. If a sheet of foolscap be held with its two leaves enclosing an angle of 20 or 30 degrees, on blowing through a tube between the leaves they close up, instead of flying asunder. The old experiment of Clement and Desormes is thus explained: a tube is fixed in the centre of a perforated disk, and a second unperforated free disk is brought up against the first. On blowing through the tube the second disk is not driven away, but on the contrary held fast until the current ceases, when it falls. In this case we have a radial spreading out of the air when it strikes the free disk, and in this radial current the atmospheric pressure is so much diminished that the excess of the external pressure supports the disk.

We have now to apply the knowledge thus gained to the deviation of projectiles. Let us suppose ourselves looking down on the bullet as it quits the gun, that the axis of rotation is vertical and perpendicular to the trajectory, and that the direction of rotation is right-handed, like that of the hands of a watch. So circumstanced, the bullet will infallibly deviate to the right of the true trajectory. If, on the contrary, the rotation be left-handed, the deviation will be to the left. In one case only can the ball rotate without having this deflecting force acting upon it, and that is when the axis of rotation is a tangent to the trajectory. *It is the object of rifling to impress upon the projectile, at the outset, this particular kind of rotation.*

But whence the deflecting force, when the axis of rotation is perpendicular to the trajectory? The ball cannot rotate without producing all round it a cyclone, by its friction against the air. On opposite sides of the bar the air of this cyclone moves in opposite directions. When such a ball passes rapidly through the air it encounters an opposing wind; and it is obvious that on one side of the trajectory this wind coincides with that produced by the rotation of the ball, while on the other side the two winds are opposed to each other, and more or less neutralize each other. The ball will move towards the side on which the strongest currents exists, the pressure on that side being a minimum. When the axis of rotation is perpendicular to the trajectory, and horizontal, it is obvious that the proper range will be exceeded when the two currents are in opposition on the under surface of the ball, a lifting of the ball being the consequence; while the range will be too short when the currents coincide on the under surface, a depression of the ball being the consequence. Thus, by the coalescence of the wind of rotation, with the wind of translation, the deviation of the ball from its true trajectory is in all cases completely accounted for.

Prior to the year 1853 rifles had been made by hand labor. A Government Committee was then appointed to determine whether they could not be produced by machinery. The committee, after due inquiry, invoked the aid of Mr. Whitworth; but he, lacking the special experience, and unwilling to modify his works to the required extent, declined the offer. But the subject remained open, and, on condition that a gallery should be built, where experiments as to the best form of rifle might be conducted, Mr. Whitworth subsequently

proposed to devise and construct the machinery necessary for the production of the *rifle barrel.* Lord Hardinge, then Commander of the Forces, obtained the assent of the Government to the proposed condition, and a gallery 500 yards long was erected on Mr. Whitworth's grounds near Manchester.

At the time now referred to the Enfield rifle was considered to be very near perfection. This Lord Hardinge pointed out to Mr. Whitworth, at the same time stating that any improvement which would induce the Government to change the Enfield for another rifle, must be "*very decided.*"

The powder then employed for small arms consisted of the siftings of cannon powder. The first step of Whitworth was to demonstrate its inefficiency. It was afterwards abandoned in favor of a quicker burning powder. A spiral groove within the barrel constituted the rifling of the Enfield weapon. The ball was elongated, being a cylinder with a conoidal front. Its length was 1.81 diameters of the bore. A wooden plug was inserted into the ball behind, which, pressed by the gunpowder, forced the lead into the grooves, and secured the desired rotation. The pitch of the spiral was such that to make a complete rotation the ball would have to pass through a length of 78 inches. Whitworth soon abandoned the rifling by grooves, and fell back upon polygonal rifling. He formed two accurately-fitting semi-cylinders, and having made the inner surface octagonal, with a certain amount of twist, he placed the semi-cylinders together, surrounded them by steel rings, and thus built up his first rifle barrel. It was only 10 inches long, but it beat the Enfield rifle. He lengthened the Enfield bullet, but found that fired from the Enfield barrel it tumbled over. He then passed from a twist of 1 in 78 to 1 in 30, then considered excessive, and with the augmented twist he was able to employ a longer bullet. With a thoroughness worthy of all admiration he passed in succession to obliquities of 1 in 20, 1 in 10, 1 in 5, even to 1 in 1. He thus exhausted the subject, and decided finally in favor of the hexagonal barrel, with a twist of 1 in 20. The length of the bullet was three diameters of the bore.

The performance of the new weapon astonished everybody. The School of Musketry at Hythe was then under the direction of Colonel Hay, who, in a report to Lord Hardinge, thus speaks of the Whitworth rifle: "The shooting with the gun on Mr. Whitworth's principle is truly wonderful. . . . The mean absolute deviation of the shots fired would not, if calculated, be above 0.65 of a foot at 500 yards; whereas, at the same distance, the mean absolute deviation of the best target of 20 shots I can produce, exceeds 2 feet." The tabulated results show an enormous superiority of the Whitworth over the Enfield rifle. "In accuracy of fire," says the 'Times' of April 23, 1857, "in penetration, and in range, the rival of the Enfield exceeds it to a degree which hardly leaves room for comparison." The best performance previously noticed had been a deviation of 24 inches in 500 yards. The Whitworth deviation at 800 yards was barely half this amount, while the deviation of the Enfield at 800 was more than twice its deviation at 500. At 1400 yards the Whitworth deviation was 4.62 feet, while at this distance a target 14 ft. sq. was not at all hit by the Enfield.

The lively interest manifested by Lord Hardinge in this inquiry was shared by Sir John Burgoyne. In a letter dated 11th December, 1857, he thus expresses his opinion of what has been achieved, and submits to Mr. Whitworth a new problem: "I am myself so fully satisfied with your musket as a *great* step in advance, and that I must expect it to be early adopted, that I am anxious for the time when we can know more about its powers of penetration of different substances, say iron, steel, elm, oak, &c., or earth, with the hard bullet" Never, surely, was a challenge more thoroughly met. The Whitworth rifle sent its bullets through thirty-four half-inch elm boards; the Enfield penetrated twelve of the boards, and was stopped at the thirteenth. But a totally new capacity was here manifested. The form of the Whitworth rifling permits of the use of a steel bullet, and with such bullets, at the time here referred to, Mr. Whitworth pierced plates of iron half an inch thick, not only point blank, but at obliquities varying from 0° to over 50°. I am not aware that up to the present hour any

other rifle has accomplished anything approaching to this.*

The conservative principle which resists all change, and which is so marked a feature of official life, may be a necessary element of greatness and strength to this country. It will, however, be admitted that resistance may be carried too far. The application of lightning-conductors to the fleet was officially resisted; and despite the foregoing results, which might well be regarded as conclusive, a committee of officers in 1859 reported to the Government that the bore of the Whitworth rifle "was too small for use as a military weapon." Augmented knowledge altered this opinion, and three years subsequently another committee reported "that the makers of every smallbore rifle, having any pretensions to special accuracy, have copied to the letter the three main elements of success adopted by Mr. Whitworth, viz. diameter of bore, degree of spiral, and large proportion of rifling surface." Finally, a special committee of officers in 1869 recommended the bore which had been recommended by Whitworth in 1857.

Call now to mind that Mr. Whitworth in 1854 found rifling by grooves the accepted method. This he changed for polygonal rifling. He found the twist 1 in 78; this he changed to 1 in 20. He found the rifle bullet 1.81 diameters in length; and this he changed to 3 diameters. The Martini-Henry rifle, which is now the favorite weapon, is rifled polygonally, a heptagon being substituted for Whitworth's hexagon. The twist of this rifle is 1 in 22, or almost identical with the twist of the Whitworth rifle, while the length of the bullet is 2.93 diameters, which is practically Whitworth's proportion. The Martini-Henry bullet is, however, rifled within the gun by the pressure behind it; and to permit of the employment of hard alloys of tin and lead, which it would be difficult to force into the angles of the heptagon, a sharp spiral projection accompanies the polygonal twist. This projection is forced into the bullet by the pressure from behind, and it gives the bullet the required rotation. The Whitworth bullet, on the other hand, is rifled outside the gun. Its inventor accomplishes by his steam engine the work which, in other rifles, falls upon the gunpowder; the brunt of this work, moreover, being borne by the shoulder of the marksman. The Martini-Henry rifle is unable to meet the condition deemed so important by Sir John Burgoyne, and to fulfil which steel bullets are necessary.

To sum up, when Sir Joseph Whitworth began his experiments, he was as ignorant of the rifle as Pasteur was of the microscope, when he began his immortal researches on spontaneous generation. But, like the illustrious Frenchman, Whitworth mastered his subject to an extent never previously approached. He found the power used for rifles unfit for its purpose. In point of precision, he obtained a "figure of merit" greatly superior to any previously obtained. He carried his ranges far beyond all previous ranges; and in point of penetration achieved the unexampled results which have been laid before you. He did this, moreover, by a system of rifling peculiar to himself, which had never been thought of previously, and which is substantially adhered to in the favorite weapon of to-day. It would be difficult to point to an experimental investigation, conducted with greater sagacity, thoroughness, and skill, and which led to more important conclusions.

It is the province of the generalizing faculty to extend to the largest phenomena the principles discerned in the smallest. With a directness of insight which he might find it difficult to define in words, Whitworth saw that the mechanical principles brought to bear in the infantry weapon were equally applicable to cannon. But here let me say that I am not too much influenced by the phrase "mechanical principles," so frequently employed by our great mechanician. These principles are as en-

* Both the plates and the projectiles which pierced them were present during the discourse. It is worth while to consider the condition of the bullet fired from the rifle in which the twist was 1 to 1. Half only of the ordinary charge of powder could be employed: the full charge tore the bullet to pieces. But with the half charge the bullet penetrated 9 inches of elm. A thousand feet a second would be a moderate velocity to assign to the bullet. It would travel, therefore, through the last foot of the barrel in $\frac{1}{1000}$th of a second; but in this length it would accomplish twelve rotations; hence on quitting the gun its rate of rotation would be twelve thousand a second. The rotating mirror with which Wheatstone, Foucault, and Cornu made their memorable experiments on electricity and light, rotated eight hundred times in a second. It is, however, not improbable that the Whitworth bullet had a still more rapid rotation than that here assigned to it; that it accomplished a million rotations per minute.

during as the universe, or rather as the mind which interprets the universe, when all the data have been embraced in the conclusion. But in practical matters mechanical principles, like wisdom, must be justified of their children; and these are the tests of experiment. It is now my duty to state to you with the necessary brevity, and with such power of illustration as I can command, how the Whitworth cannon has borne the experimental tests to which it has been subjected.

Before you is a piece of armor plating 4 inches thick, manufactured by an eminent firm, with the knowledge that it was to be tested with the Whitworth gun. Beside it is an elongated, flat-headed, rifled projectile, hardly exhibiting the least mark of distortion. Yet the plate has been perforated by that steel bolt. The gun which fired the bolt was a 12 pounder; but owing to its elongation the actual weight of the projectile is 29 lbs. This assuredly was a great achievement for so small a gun. You have seen what the Whitworth rifle accomplished with steel bullets. Before you is a series of parallel results obtained with the Whitworth cannon. The plates here presented to you have been perforated, not only by direct impact, but at obliquities of 35°, 45°, and 65°. This, to an outsider like myself, appears to be a result of extreme importance; and the reason I say so will be immediately clear. The projectiles which did this work are flat-headed. But I have now to direct your attention to two other plates which have borne the impact of two ogival pointed shot, the one of "chilled" metal, the other of steel. Instead of being penetrated, the plates are indented merely. Fired at an angle of 45° these pointed shots glanced from the plates, scooping out a small hollow, instead of piercing them. In this experiment, the chilled shot broke up into fragments, but the steel one is intact. Taking a steel bolt in the hand, and urging the edge of its flat front obliquely against an iron plate, the bolt is arrested, because the edge cuts the plate like a chisel; urging the pointed shot at the same obliquity against the plate, it glances off. Thus, a mere hand-experiment shows the difference between the flat-headed and pointed shot, when the incidence is sufficiently oblique. And as in actual warfare oblique incidence will probably be the rule, and perpendicular incidence the exception, this demonstrated power of the flat-heated bolt to penetrate, when the pointed shot fails, seems to me worthy of the most serious consideration.

Almost equally instructive are the experiments executed to determine the comparative power of flat-headed, round-headed, and pointed projectiles to penetrate water at an oblique incidence. The angle of depression was 7° 7′, and the length of water to be penetrated was 80 inches, the mark aimed at being 10 inches below the water line. The flat-headed projectiles appear to have gone almost directly to the mark; the round-headed ones were tilted up and struck the plate just below the water line; while the pointed or ogival ones were completely ejected from the water, and struck the plate at 9 inches above the water line. These are points regarding which, one would think, no uncertainty ought to exist; they ought not to rest on the unsupported testimony of any inventor. They ought, one would think, to be tested by independent men, refuted or confirmed; at all events, appraised at their proper value and significance.

I stand here to-night, not as the advocate of any particular system of ordnance, but in response to an expressed desire that you should be made acquainted with the physicial experiments executed in connection with this subject. To others I leave the task of deciding on the national importance of these experiments. But I claim the right, if I deem it necessary to do so, of not only presenting the facts, but of expressing the thoughts which have occurred to me during the preparation of this discourse. From all that I have read and heard, I can come to no other conclusion than that for direct impact the ogival, or pointed projectile, possesses a higher penetrative power than the flat-headed one. Hence some of the statements made by the eminent mechanician who advocates the use of the latter must, I think, appear to a severe inquirer too unqualified in their assertion of its superiority. It is demonstrably superior at the oblique incidences above referred

to; but from the comparison instituted by Sir Joseph Whitworth himself, and recorded in 'Guns and Steel,' pp. 46 and 47, it is evident that in the case of direct impact the ogival head exceeds the flat head in penetrative power. At an angle of 45° the latter clearly asserts its superiority. Hence, as the pointed head is best for penetration at an obliquity of 0°, and as the flat head is best at an angle of 45°, the thorough experimental examination would have to determine the obliquity at which both would be practically equal. The knowledge thus obtained, taken in connection with the exigencies of actual warfare, would be an important factor in the decision of the comparative merits of the two forms of projectile.

In the Preface to 'Guns and Steel' Sir Joseph Whitworth gives the following summary of his experiments on penetration:

"In 1857 I proved, for the first time, that a ship could be penetrated below the water line by a *flat-headed* rifled projectile.

"In 1860 I penetrated, for the first time, a 4½-inch armor plate, with an 80 lbs. *flat-headed solid* steel projectile.

"In 1862 I penetrated, for the first time, a 4-inch armor plate, with a 70 lbs. *flat-headed steel shell*, which exploded in an oak box supporting the plate."

(I may remark that I was present when this experiment was made, and can testify that its results astonished those who witnessed it.)

"In 1870 I penetrated, with a 9-inch bore gun, three 5-inch armor plates, interlaminated with two 5-inch layers of iron concrete.

"In 1872, with my new 9-pounder breech-loading gun, and a flat-headed steel projectile, I penetrated a 3-inch armor plate, at an angle of 45°.

"All these performances," continues Sir Joseph, "were the first of their kind, and were made, with one exception, with flat-headed projectiles." This exception, however, must be the important one, in which 15 inches of armor plating, and 10 inches of iron concrete, were penetrated by (I suppose) an ogival pointed shell. This, I take it, is the experiment described in pp. 51 and 52 of 'Guns and Steel.' It confirms the conclusion which I have been compelled to draw from the other experiments of Sir Joseph Whitworth above referred to. I could wish that this superiority of the ogival point, in the case of direct impact, had been more distinctly recognized; but I also wish to remind those whom I address that this is a point of detail, which in no way affects the merits of the Whitworth gun.

The tentative skill and insight which were so conspicuous in Mr. Whitworth's experiments with the rifle, are not less conspicuous in his experiments on rifled cannon. He takes the various elements of the gun in succession, and determines for each its condition of maximum efficiency. As before, he pushes his experiments through a range of variation so wide as to embrace both sides of this maximum. In 1856 he demonstrated that a rifle bullet should be 3 diameters long. "The rule," he affirms, "holds good for a 35-ton gun, as well as for a rifle." His experiments on the relation of the amount of twist to the length of the projectile are in the highest degree interesting. Projectiles varying from 1 to 7 diameters in length, yielded the following results:

"With one turn in 10 inches, all the projectiles went steadily with the point first.

"With one turn in 20 inches, the projectile became unsteady when more than 6 diameters in length.

"With one turn in 30 inches, it fell over when more than 5 diameters in length.

"With one turn in 45 inches, the projectiles turned over, end flew very wild, when more than 3 diameters in length."

The conclusion drawn from these experiments was, "that unless a gun be rifled with a quick pitch, so as to give a high rotation to the projectile, it would not be possible to fire long projectiles." With a sufficient twist, Mr. Whitworth succeeded, in 1856, in firing projectiles 10 diameters in length, and weighing 150 lbs. For range, the best form of projectile has a conoidal front, a slightly tapered rear, and is from 3 to 4 diameters long. With high elevations, the flight of such a projectile may exceed, by a mile, that of one with its rear untapered.

The greatest range hitherto obtained was reached at Shoeburyness in 1868.

It amounted to 11,243 yards, or nearly 6½ miles. The angle of elevation was 33°; weight of gun, 14 tons 8 cwt.; bore, hexagonal; major diam., 9 inches; minor diam., 8.2 inches; pitch, 1 turn in 165 inches; weight of shot, 250 lbs.; length, 24.5 inches; powder charge, 50 lbs.

The possibility of attaining so great a range depends on the weight and cross-section of the shot. Were there no atmospheric resistance, the range would depend solely on the velocity of the projectile on quitting the gun. But if two projectiles, one possessing a greater transverse section than the other, start with the same velocity, the thicker bolt, encountering greater resistance, is brought more rapidly to rest. The velocity at starting may be even greatly in favor of the thicker projectile, while at long ranges it is left behind.

And here we are met by one of those practical reflections which force themselves upon the thoughtful mind, and in regard to which I have already claimed liberty of expression. If the object of the artillerist be solely to throw his shot with the maximum precision, to the greatest distance, then, so far as the data before me enable me to judge, Sir Joseph Whitworth has made out a conclusive case. In the common rifle, where each bullet is intended to kill or maim a single man, these two elements of range and precision are paramount. But in artillery practice another consideration comes into play. The use of shrapnel shell is one of the most important features of such practice. In this case vastness of range is not the only thing sought. At a certain point in its trajectory—a point which the practical artillerist might fix at two, three, or four thousand yards—the projectile has to burst, and spatter a rain of bullets round it. The effectiveness of such a projectile must obviously depend on the bursting charge which it is able to carry, and the number of bullets which it is able to scatter. If—and bear in mind that I use the "if"—if the projectile be so attenuated as to diminish seriously the bursting charge and the number of bullets, then it is easy to see that while it might assert a clear superiority as regards length of range, it might be less effective than a projectile of greater diameter, at the distances most advantageous for the use of shrapnel shell. I am far from saying that the Whitworth shell is unable to fulfil all the necessary conditions; but I do say that the shrapnel shell raises a question which was not raised in the case of the rifle. And were I on a committee entrusted with the decision of this question, I should require it to be proved that the very perfection of a gun, regarded from one point of view, is not an imperfection when regarded from another. The question is one for experiment to decide; and what those interested in the establishment of the truth will be most inclined to deprecate, is the closing of the door against experiment.*

Sir Joseph Whitworth has always advocated the use of steel in the construction of guns; but without some guarantee of its trustworthiness, he could hardly have expected to convert the world to his views. It would be unreasonable to expect military authorities to make their guns of a metal which, through some defect impossible to guard against, might at any moment convert the gun into a shell, scattering ruin among those who trusted it. The onus therefore rested upon the advocate of steel, to produce a metal which could be relied on. With the tenacity of purpose, and fruitfulness of inventive skill which characterized his whole previous career, Sir Joseph Whitworth attacked this problem. The solution of it will, perhaps, be best understood by giving you an account of an experiment which I witnessed at Manchester.

Within a hollow steel cylinder, of enormous strength, were placed a series of cast-iron bars, so as to form a kind of lining. The bars were laid loosely side by side, so as to admit of the passage of a gas between them. They were also grooved, with a view of facilitating gaseous motion. The bars were coated by a porous lining of sand and other materials, through which gases could readily be driven by pressure. In the middle of the cylinder stood a core, also formed so as to permit of the escape of

* Length of range, in the case of elongated projectiles, being obtained through the diminution of atmospheric resistance; the *fighting distance* at sea becomes a point of cardinal importance in reference to this question. If this distance be great, the elongated projectile triumphs; but if it be small, a short projectile with a high initial velocity, might be preferable.

gas from it. A space of several inches existed between the inner core and outward sheath. A large ladle was at hand, and into this was poured the molten metal from a number of crucibles. From the ladle again the metal was poured into the annular space just referred to, filling it to the brim. Down upon the molten mass descended the plunger of a hydraulic press. On first entering it a shower of the molten metal was scattered on all sides; but inasmuch as the distance between the annular plunger and the core on the one side, and the sheath on the other, was only about one-tenth of an inch, the fluid metal was immediately chilled and solidified. Thus entrapped it was subjected to pressure, which amounted eventually to about six tons per square inch.

Doubtless gases were here dissolved in the fluid mass, and doubtless also they were mechanically entangled in it as bubbles. I figure to myself the fluid metal as an assemblage of molecules, with the intermolecular spaces in communication with the air outside. Through these spaces I believe the carbonic oxyde and the air to have been forced, finding their escape through the porous core on the one side, and through the porous sheath on the other. From both core and sheath issued copious streams of gas, mainly, it would seem, in the condition of carbonic oxyde flame. A considerable shortening of the fluid cylinder was the consequence of this expulsion of gases from its interior. The pressure was continued long after the gases had ceased to be ejected; for, otherwise, the contraction of the metal, on cooling, might subject it to injurious internal strains. In fact, castings have been known to be rent asunder by this contraction. By the continuance of the external pressure, every internal strain is at once responded to and satisfied, and the metal is kept compact.

The two main factors which determine the quality of any kind of steel are its strength and ductility. The method adopted by Sir Joseph Whitworth in determining these factors is, like all his mechanical contrivances, admirable. Both ends of a cylinder of a definite length and cross section are screwed firmly between two jaws, which are then separated by hydraulic pressure. For a time this stretched cylinder maintains a uniform diameter. At a certain pressure it passes its limit of elasticity, the passage being distinctly indicated by the dial which registers the pressure. From this point forward the cylinder is observed to contract at its centre, and it finally snaps across. The "strength" is measured by the breaking force; while the "ductility" is determined by bringing the fractured surfaces close together, measuring the length of the stretched mass, and expressing its elongation as a percentage of the original length of the cylinder. In one experiment made in my presence, forty seconds sufficed to stretch and break the cylinder, and there was not the slightest jar or jerk observed during the process. I entirely sympathize with the desire entertained by Sir Joseph Whitworth that these two elements of strength and ductility should be determined for, and registered upon, every ingot and bar of steel employed in the construction of our railway tiers and axles; and, indeed, on all portions of machinery the giving way of which imperils human life.

In the western mining districts of this country an unusual method of conveyance has been adopted. A wooden aqueduct, called a "flume," is constructed of triangular section, six feet wide and some three deep at the centre, requiring but slight water pressure, and following the natural inclinations or sinuosities of the district or valley along which timber has to be conveyed from the forest to the mine. At about every second mile a guardian is stationed to remove any obstructions which may occur, but although many of these flumes have been in operation for the last two or three years, some of them of a length of 50 miles, blocks very seldom occur in the passage of timber along them. They afford means of transit for foresters, who seat themselves on the floating piles of wood. Mr. Watson, Secretary of the British Legation at Washington, mentions, that in traveling through Nevada by railway he observed a snow shed or tunnel constructed entirely of timber, which it took 1 hour and 20 minutes to traverse, and which is said to be 29 miles in length.

PREHISTORIC METALLURGY.

By W. MATTIEU WILLIAMS, F.R.A.S., F.C.S.

From "Iron."

Mr. St. John V. Day, in his learned and interesting series of papers recently communicated to Iron on "The High Antiquity of Iron and Steel," endeavors to show that the art of reducing iron ores and the use of iron are of much greater antiquity that has hitherto been supposed, and also that the order of the tool-making progress of the human race has not been, as usually stated, from stone to bronze, and from bronze to iron and steel. He tells us that he has found evidence "in proof of the claims for iron to be considered amongst the earliest, if not the very earliest, of materials used by the human race," and which "negative the popular and too hastily-drawn conclusion, that man did not commence to use iron until after whole milleniums of dealing with bone, stone and bronze."

In spite of all the recent discoveries of archæologists, of Mr. Day's conclusions, of Sir John Hawkshaw's recent address, and of the comments that have been so freely made upon it, in which the subdivision of the prehistoric human progress into the stone, bronze and iron periods is treated as an old-fashioned and superseded theory, it appears to me that the balance of evidence is decidedly in favor of the conclusion that man used only stone implements long before he learned to use metals, and that, generally speaking, the use of bronze preceded that of iron and steel. I refer merely to the order of discovery and of use, without attempting any approach to measurement by years of the age and length of these periods.

The facts stated by Mr. Day and Sir John Hawkshaw unquestionably prove a higher antiquity to the use of iron than was formerly assigned to it, but we must collate these facts with the corresponding modern discoveries, which prove a vastly higher antiquity than was formerly assigned to the *début* of man upon the earth, and which antedate proportionally the stone and bronze ages as well as the beginning of iron. We now know that flint-working savages were certainly living in England during the interglacial, and probably during the pre-glacial period of tertiary geological history, that they existed here, in company with many animals that are now quite extinct. There are good grounds for supposing that they were then driven southward by the desolation of the slowly advancing ice-coat of Northern Europe, and that when the upper tertiary deposits of Asia, Africa and Southern Europe are as well explored as those of Britain, we shall know a great deal more about them. The monumental records that are quoted by Mr. Day and Sir John Hawkshaw, although of very great antiquity when compared with the age of written history, belong to what is quite the present period of the geological record; or, restricting our view to the geological time during which man has existed upon the earth, the pyramids, the Hindoo ruins and other Oriental monuments, wherein these earliest vestiges of iron have been found, all belong to relatively modern times. The recent discoveries which push back the date of man's entry upon the world to the time of the deposition of the newer pliocene rocks, and associate him with the mastodon, the mammoth, the dinornis, the *bos primogenus*, &c., have rendered necessary a complete readjustment of our ideas and measures of antiquity. Admitting, with Sir John Hawkshaw, that the piece of iron found in the great pyramid must have lain there at least 5000 years, and that iron was made in India a few thousand years before this, we are still dealing with dates that are quite recent in comparison to the period when the bones and implements of the primitive brachycephalous Britons were buried under the glacial drift. Our ablest direct investigators of this portion of the geological record will, I believe, hold me guiltless of exaggeration or unwarrantable assumption, if I assert the strong probability of the existence of paleolithic

savages on some of the warmer regions of the earth at the time when the very site upon which the pyramids are built was a broad estuary covered by the salt water of the Mediterranean ; when the deserts of Gobi and Shamo, the plains of Yarkand, Tarim, &c., were covered by the waves of a vast high-level Mediterranean, and a large portion of the plains of Hindostan, China and Mesopotamia, from which Mr. Day derives some of his ancient iron relics, were yet in course of deposition from the turbid estuaries of the Ganges, Indus, Hoang Ho, Tigris and Euphrates.

At any rate it may be affirmed with perfect safety that recent investigations have pushed back the antiquity of iron to a *great* extent, that of bronze to a *considerable* extent, and the stone period to a *vast* extent. The interval between the beginnings of bronze and that of iron may be relatively smaller than was once supposed, while, on the other hand, the duration of the stone period must have been vastly greater, as the general result of modern research is to reveal its further and wider extension and to antedate its beginnings more and more profoundly.

Although our ideas of time, our positive measurements, and the idea of equal or similar periods which the bare subdivision of human progress into three ages naturally suggests, may require rectification ; nothing has yet been discovered that demands any radical change in the accepted statements of the *order* of the developments of human metallurgical achievements. There may be *gaps* in the series, but there is no *inversion*. The discovery of localities where iron implements are lying side by side with those of flint and without any traces whatever of the intermediate bronze, only shows that these places may have been uninhabited during the bronze period, or that its paleolithic or neolithic savages may have been elevated or superseded by an irruption of steel and iron sword bearers.

An exact analogy of this is presented in the geological series. Certain strata or groups of strata are wanting in many places, either because these localities were up-heaved during the epoch of deposition of such strata, or because they have been denuded subsequently. Different portions of the globe have simultaneously existed in different stages of progress, but there is no instance known of an inversion of the order of succession of life in any part of the world.

Philosophically regarded, this is the most important of all the results of geological research ; it is the basis of the doctrine of evolution. The invariability of the order of human metallurgical progress has a similar interest ; it is, in fact, a continuation of the same record. It shows that the human being is essentially a progressive animal—brings the great question of human progress under the domain of natural law. If it could be shown that the human race had ever gone back from iron to bronze and from bronze to flint, the general conclusions of modern science concerning the order of human development might be justly challenged in favor of the old traditional ideas on this subject.

The usefulness of bronze tools is so much greater than that of stone implements, the superiority of steel and iron to bronze is so decided, and their applications are so essential to human welfare, that a return from steel and iron to bronze, or from bronze to stone, would indicate a positive incapacity for progress, and an inherent tendency to retrogression on the part of the people capable of such return, while, on the other hand, the fact that no such return can be demonstrated is, as I have already said, the broadest and most fundamental expression of the law of human progress. Such apparent exceptions to the law of metallurgical development as the discovery of bronze surgical instruments at Herculaneum, merely shows that for special purposes the old material may have been preferred in comparatively recent times. Facts of this kind interlace the three periods in such a manner as render any broad and definite boundary unattainable, but do not reverse them. The same occurs in reference to geological periods. The discovery of living cretaceous foraminifera in the deep Atlantic, and their operation in depositing a variety of chalk at the present moment, is an illustration of this.

The manner in which a gap in the metallurgical series may occur over a limited area is instructively shown by

the present state of things in New Guinea, the natives of which, as far as their own productions are concerned, may be said to be still living in the stone age, as the bottom of the deep Atlantic is still in the Cretaceous geological period. But side by side with their own stone implements are a few hatchets and rude spear-heads, &c., made of iron scraps that the natives have obtained from their rare European visitors. They covet these so eagerly that they will exchange an elaborately carved paddle or club for a piece of old iron hoop. They have no bronze implements, not because they used iron before bronze was invented, but because the stone period was in their case so far prolonged that they only began to use iron after bronze implements were superseded. This may have occurred at other places than New Guinea and at earlier times than the present.

This curious prolongation of the stone period into the nineteenth century is a fact that should teach us to be cautious in making any sweeping inferences from the discovery of particular and exceptional monumental or other human remains. If a collection of the home-made implements of the present inhabitants of New Guinea were laid before an archæologist, with no further information than these afforded, he would make some curious mistakes in attempting to determine the period of their origin.

We are therefore justified in bringing natural metallurgical considerations, or, I may say, metallurgical necessities, to shed additional light upon the obscurities of these archæological problems.

I have already ventured to do this by showing how the natural properties and the distribution of the metals must have influenced the order of their discovery and use by man. These considerations point to gold as the first metal that would offer itself to human notice, for the following reasons:—(1) It exists naturally in the metallic state. (2) It is found upon the surface of the earth, just in those places where man would first settle, viz., on the banks of rivers and in alluvial deposits, and although only found in small quantities it is very widely distributed. (3) It is so brilliant in its native condition that it could scarcely fail to attract attention, and its malleability is such that it may be shaped by the rudest stone implements. Its discovery would of course begin with a few rare nuggets, and it would probably be first used for ornament. Savages knowing the use of fire would soon discover its fusibility, and then might collect smaller fragments or grains and melt them together.

The use of gold and the idea and valuation of metallic lustre would at once direct attention to the yellow gold-like iron and copper pyrites. Attempts to fuse the first of these would fail, but if the second were heated in a brisk wood fire it would be roasted, fused, and, more or less completely, reduced to the metallic state by the driving off of sulphur.

But why, it may be asked, should the discovery of the other constituent of bronze—the tin—have taken place so early, seeing that tin-stone has no metallic lustre and is scarcely distinguishable from ordinary pebbles?

My reply to this is that tin-stone, like gold grains and nuggets, is spread by the action of rivers upon the surface of the earth; and, although less widely diffused than gold, is much more abundant where it does exist, and is just on the banks of rivers and alluvial deposits where man would first settle. The charred wood of the kitchen middens, &c., shows that the men of the later portion of the stone period had learned the use of fire; but as their rude earthern pots and laborously chiseled stone troughs and basins would not bear firing from below, they probably adopted the well-known primitive method of boiling water by throwing into it pebbles that had been heated in a wood fire. The Chersonese or Cornish savage could not have performed this operation a dozen times—could not have cooked a single grandmother—without deoxydizing a few tin-stone pebbles, fusing the metallic tin, and finding brilliant silvery metallic buttons among the ashes of his wood fire. As copper and tin ores are usually associated, he would probably have also produced an accidental bronze in the course of his cookery. The same would occur in the tin-stone districts of Sweden, Bohemia, Saxony, Hungary, &c., in most of which nuggets of copper ore also exist among the river-borne pebbles. In

the few localities where argentic pyrites occurs superficially, buttons of silver may have been similarly obtained.

The comparative infusibility of iron would render its accidental discovery by a savage people, otherwise unacquainted with metals and metallurgy, a practical impossibility. When, however, they had so far advanced as to imitate deliberately the processes of reduction and fusion that had been discovered accidentally, and had carried on their metallurgical operations far enough to have invented some rude form of bellows to urge the fire; when they had made castings and experimented upon the properties of alloys, they would probably succeed in obtaining first a fusible sulphuret of iron, or rather sulphuretted iron, from the attractive yellow pyrites; and this product, in spite of its red-shortness, would have considerable value to such people.

Once familiar with this dull, gray, semi-metallic substance, liable to become reddened on the surface by exposure to the air, the resemblance of native magnetic oxydes, of some of the hæmatites, and of specular iron ore, to it in color, semi-metallic lustre and density, would naturally lead to attempts to fuse these ores in the primitive furnaces urged by the rude bellows then in use.

What would follow such an attempt carried on with some perseverance? Nothing less than the production of a fusible, highly-carburetted cast steel, of great hardness and fine quality. With pure ores, wood fuel, and not too strong a blast, neither malleable iron, nor that grossly impure steel to which we give the name of pig-iron, could be produced. The eagerness to acquire a cutting weapon or implement, would soon lead to the discovery of the leading properties of this steel. A people already acquainted with bronze that can be hardened by heating and cooling slowly, or softened by heating and plunging while red hot into cold water, would of course try the same upon their new gray metal, and would thus discover that by opposite means it could be made capable of cutting and forging itself. This discovery of steel and subsequently of malleable iron would place in the hands of man a new element of physicial power, and one of such great value and importance that its introduction would constitute the widest stride that had yet been made in the course of human progress. It would truly open a new era, at a time when physical power was all in all, and therefore we are justified in thus describing it, and in speaking of the stone, the bronze and the iron ages as definitely and distinctly as the geologists refer to the primary, secondary and tertiary geological eras, and of the paleozoic, mesozoic and cainozoic periods of animal evolution.

Continental Coal.—The prices of coal, which tend to fall in England, are steadily falling in Germany as well, there being nothing to indicate any probability of a rise. On the other hand, the coals of France and Belgium, even for the district of Liége, have a very distinct upward tendency and show firmness. At Mons, in the coal county of Hainaut, this is more especially marked, prices having hardened, not in consequence of diminished production, but in consequence of greater commercial activity. This is partly through the abandonment by the Paris Compagnie de Gaz of German coals, which gave unsaleable coke. The Société John Cockerill has reduced its colliers' wages by 10 per cent., and the Marihaye Company has made a still larger reduction. In the Departments of the Nord and the Pas de Calais the coal-owners have subscribed 6,000,000 francs for improving the canals of two departments. The improvements will be carried out by the local administration, and will result in giving the canals a navigable depth of over 10 feet. This movement has probably been stimulated by the establishment at Paris of various agencies for Newcastle coal. In the first six months of the present year coal was imported into France to the value of 94,097,000 francs., being an increase in value upon the corresponding period of 1874 of over 14,000,000 francs. The total coal raised in the two departments of the Nord and the Pas de Calais during the first six months of 1875 was 32,120,715 metric quintals, being an augmentation over the corresponding period of 1874 of 2,600,585 quintals.

HOW THE PARISIANS BUILD A HOUSE IN FLATS.

From "The Architect."

THE newspaper writers of more than one country, who used to compare Louis Napoleon to Octavius Cæsar, had at least foundations strong enough to support a superstructure more solid than a compliment. If the latter was enabled to say that he had changed Rome from a city of brick into one of marble, the French courtier who declared that the Emperor had found Paris of plaster and would leave it of stone, told a truth which neither the friends nor detractors of the Second Empire seem to have even yet completely realized. But the rearrangement and reconstruction of Paris has been proceeding gradually since the great Revolution. Much of that which was achieved by Napoleon III. had been projected by his uncle; although even in the time of Louis Phillippe the majority of the streets had no foot pavement, and the gutter was in the middle of the roadway. In lighting as well as cleanliness the French capital was then far behind London. It is incredible to hear that thirty years ago the Parisians envied the Londoners their large open squares, their lamp posts, and their vestry-men! To those who know the two cities as they are now, Macaulay's celebrated description of the English metropolis in his chapter on the "State of London in 1685" appears an exaggerated eulogium; but when it was written, London in all but its architecture was a finer city than Paris.

The house in flats specially constructed for the purpose was not common in Paris until the end of the reign of Louis XIV., who died in 1715. The custom in France of building houses for the express accommodation of several families does not go back further than the seventeenth century. A birdseye view of Paris engraved upon copper in 1607 shows the cathedral towering above a number of one-story tenements; and the churches and public buildings stand out from a mass of houses of small elevation. I am constrained to think that the intercourse then existing between France and Scotland led to the adoption in Paris of residence by floors, as it was practised in Edinburgh even by families of noble birth in the sixteenth and seventeenth centuries. Facts go to prove that the many comforts which the Parisians now obtain are of recent invention. The introduction of back staircases into Paris houses in flats dates only from the beginning of the nineteenth century; and I think it probable that the Parisian *maison-a-loyer* only received its present developed form after the great Revolution.

The recognized authority in Europe upon this kind of habitation is M. Cesar Daly, the well-known author of books upon this and cognate subjects. He divides Paris houses into three heads according to classes of the population: 1st, The aristocracy, who live in private houses (*hotels*); 2nd. The *bourgeoisie* or middle class, who occupy houses in flats (*maisons-a-loyer*); 3rd, The working class who inhabit houses from which the two classes above them have departed. As M. Daly has poetically said that the house is the "family vestment," so, I may venture to add, the worn-out clothes of the few descend to cover the many.

Various fallacies are rife amongst Englishmen concerning Parisian dwellings, and people frequently talk about the ten-storied houses of Paris. None of more than five stories above the ground floor and a roof have been built there since the year 1784. It is popularly supposed that in Paris noblemen occupy the first and second floors of a house, gentlemen the third and fourth, tradesmen the fifth, and working men the attics! But Paris houses are inhabited by people of nearly equal position in society, or, at least, they are so arranged by means of separate staircases, that the meaner tenants do not encounter their happier neighbors except in the open court or at the carriage entrance. It has been said that the roof to which the elder Mansard gave his name was invented to evade the laws regulating the height of buildings in Paris, but the mansarde or curb roof had been in fashion

* A paper read before the Architectural Association by Mr. W. H. White.

for 150 years before any such laws were made. From 1784 to the present day the legal height of a Paris house has depended upon the width of the street in which it was built. The facades of houses erected on the boulevarts and principal streets must perforce be composed of freestone. The floors are of rolled iron enveloped in plaster concrete, and the general application of iron to the construction of floors is due to the carpenters, who in 1840 "struck" in a body, and thus solved for their city at least the problem of fireproof construction. These floors are no man's specialty or patent. The rolled iron joists can be laid by any smith, carpenter or *macon*, and finished in plaster by any workman of the last-named trade. An order from the architect is alone necessary, and to people about to build in Paris an architect is absolutely indispensable. There are no general contractors in the French capital. On an ordinary Paris house there is rarely a clerk of the works. The foreman of the *macons* in conformity with tradition, is the head of all the workmen of all trades. In France, the *macon* is not a worker in freestone like the English mason; he is bricklayer, plasterer, builder of rough stone walls, worker in cement of all kinds. The contractor for all these works is also entrusted with the masonry, as the word is understood in England. He erects the scaffolding, and only in large buildings is the carpenter employed in such work. All work is paid for by measurement after completion, according to a municipal tariff which varies annually and forms a basis of prices for artificers' work done within the city of Paris. The architect has to treat with, say, a dozen tradesmen, innocent of any sort of mutual organization and unwilling to accept instructions from any but their master—the master of the work. A dozen contractors imply a dozen sets of drawings, a dozen agreements, a dozen bills. The architect has to verify the items of each bill, to check the dimensions and modify the prices for labor and materials in accordance with the signed agreements and the Parisian tariff. He consequently employs a specialist (*verificateur*), whom he pays out of his usual commission sometimes as much as one per cent.; but the architect alone is responsible to his client for the accuracy of these bills, the name of the suveyor he employs not appearing in the transaction; and a French architect's responsibility to his client and to the State endures for ten years after the completion of the works he has superintended. I propose to describe the erection of a first-class house in flats; with appliances which have been practically tested by myself, but the house will be a fancy picture, sketched at different times and from several examples.

The plans, sections, and elevations, are ready any lithographed for the use of the contractors and foremen. The line of frontage is obtained, and to get one's line of frontage on a Paris boulevart is a thing quite as serious as getting married or buried. The master-mason is ordered to begin, and his foremen, that is, the master-companion, marks out the trenches to receive the foundation of the basement walls. But below these are two large cubes of earth to be excavated, for the two cesspools required to drain two separate sets of water-closets—one set for the large residences, and one for the servants and a few small residences. A cesspool must be immediately under the water-closets it serves. The main drain pipe must descend vertically into it, and ascend vertically to a certain height above the ridge of the roof. A junction pipe, connected with the water-closet apparatus, enters this main a little below the level of each floor. In juxtaposition to the main is placed another pipe, which ascends vertically from the cesspool to above the roof, this pipe being used exclusively for ventilation. Both these pipes are attached to a strong wall with hoop-iron, and completely enveloped in cement. The walls, bottoms, and vaults of the cesspools must be built of *meuliere*—a species of reddish petrified sponge, and very hard. It is found near Paris, and it affords an excellent surface to receive the coat of cement with which the walls internally of all cesspools must be covered, their internal angles being rounded. From the vault of each cesspool a shaft must be formed, the top of which must communicate with the court-yard, and be covered with a stone slab fitted into a stone frame. This slab is hermetically sealed, and only opened when the cess-

pool is full; and then it cannot be refixed until the interior has been officially inspected. These cesspools are so built that the top of their vaults is on a level with the floor of the basement story. While they have been building a gang of men have been occupied at a corner of the site. They are preparing *beton* which, according to learned professors, is of Roman origin, and which I venture to say is less easily adulterated than the English concrete. In the corner alluded to a platform has been made of planks, on one side of which is a heap of pebbles and chips of stone, on the other a heap of sand, and under a shed close by a number of sacks containing hydraulic lime; and some mortar is already prepared for use. Wheelbarrows are scattered about, and the bottoms of some of these are made with iron bars to hold the pebbles, for it is necessary that these be thoroughly washed, and consequently when they are placed in the wheelbarrows the water escapes from them through the iron bars. My *béton* shall be perhaps extravagantly good; and, as is well known, *béton* is a mixture of hydraulic mortar with pebbles or stone chips from the quarries and yards about the size of an egg and similar to those used to macadamize a road. The mortar and pebbles are sometimes very well mixed by a machine, but it is better done by hand. Five wheelbarrows are required, three with bottoms of iron bars, and two of ordinary make. The three first are filled with pebbles and the two others with mortar. One barrow of pebbles is wheeled to the wooden platform, and its contents are spread out over the entire surface; above these is uniformily spread a barrow of mortar. A second barrow of stones is deposited in the same manner and a second barrow of mortar, which is then covered with the third barrow of stones. The heap is turned over and dragged with a three-pronged fork or hook; and the pudding is considered to have been sufficiently stirred when the pebbles are entirely enveloped in mortar. Before throwing the *béton* into the trenches a layer of gravelly sand has been spread over their bottom surface, which will prevent the mortar from becoming deteriorated with the soil beneath before it is properly set. There is no absolute necessity to ram *béton* if only it be carefully laid. I have ordered it to be spread in horizontal beds of from eight to ten inches thick, and I shall begin the walls upon it almost immediately. No *béton* has been put under the walls of the cesspools because the building is not to be supported upon them. There is a height of about 30 inches of *béton* under the principal walls, and when they have reached a few feet in elevation I shall spread over the whole area of the building a layer of *béton* about 10 inches thick, the top of which shall be level with the top of the *béton* under the principal walls. Thus I can support the 9 inch brick partitions to form the numerous coal and wine cellars, which occupy the whole of the basement story, for in Paris an underground story is seldom, if ever, inhabited. The walls which touch against the earth will be built of rough hard stone (*moëllons*) at least two feet thick coated externally with cement. These walls are now within a few inches of the street level; brick arches are being turned over the cellars between the partition walls, their flanks are filled up with more *béton* and a general even surface is obtained about a few inches below the finished level of the ground story. A horrible odor pervades the works. Around a cauldron two men are dancing. One stops to throw into it a lump of bitumen, then a little gritty sand, and the other stirs up its contents. Suddenly one rushes off with a saucepan full of the boiling liquid and spreads it cautiously over a part of one of the walls; then a bricklayer lays bricks hurriedly upon this same spot, blasphemes and sucks his fingers. More asphalte is brought, always in a boiling state, and eventually two courses of hard bricks are laid over the principal walls. A coat of asphalte about an eighth of an inch thick will be ultimately spread over the whole area of the ground floor, under stone, tiling or parquetry as the case may be. Above this level I have seven stories including the roof to construct, and the existence of a shop on the ground floor necessitates as much open space as possible. The weight of the internal walls must be concentrated upon a succession of supporting points or piers which I shall build in solid blocks of hard limestone. There must, however, be a carriage en-

trance and the walls enclosing it must be of solid stone, so that the difficulty of making a house-front appear logically constructive is lessened in Paris by the fact that such entrances form an important portion of the front. The rest of the front will be surported by coupled iron columns placed between stone piers. The girders they support are of rolled iron, also coupled, and about 10 inches deep, which will suffice to carry my facade of 50 feet of masonry. These girders are secured together with wrought iron collars about four feet apart. Planks having been temporarily placed under them they will be filled with bits of broken brick, lumps of old plaster partitions, &c., and the whole "run" in fresh plaster. A solid lintel of iron and plaster is thus formed, about 18 inches thick, 11 inches deep, and from 6 to 8 feet long. Similar lintels are being placed over all the internal piers; and upon them the rolled iron joists forming the floor of the first story will be laid. These lintels will also receive the weight of the partitions of the different stories over. The completed flooring, including the ceiling and parqueting, will not exceed 12 inches in thickness. The rolled joists are placed 30 inches apart, and, at right angles to them, are fixed wrought iron bars which are bent at the ends so as to clip the upper flange of the joist and permit the bottom of the bar to be level with the lower flange of the joist. These bars are placed about 3 feet apart, and as much as possible in a straight line, for they will act as a strut. A blow suffices to fix them. Upon these bars, in lines parallel with the joists, and of the same length, light iron rods are laid. All this iron has received two coats of paint (of *minium de fer*), and the skeleton is ready to be converted into a concrete floor. A temporary ceiling of planks is erected under the iron skeleton. Upon this planking are thrown bits of old plaster, brick, and hard rubbish of all kinds until the whole space between the joists is filled up almost to the level of their top flanges. Liquid plaster is then run into and over the whole mass of iron and ballast, and the morning after the temporary planking may be removed. But this useful substance, plaster, has a tendency to swell, and if a small cavity be not left between the solid plaster floor and the stone wall, the latter will be pushed outwards. It is usual at the level of each floor to tie in the external and party-walls with iron bands looped at different points over circular iron rods (*ancres*) inserted vertically in the centre of a solid block of stone. These bands are hooked together midway between the rods. For my part I only use these ties in deference to the vested interests of the master-smith; for in spite of the usual precaution of paint the insertion of iron chains into soft limestone is a hazardous proceeding. Damp rusts the iron, which then increases in bulk and cracks the stone; or it is ultimately reduced to carbonate of iron.

The building is now at the level throughout of the first flat, which is usually called the *entresol*, and the walls have reached to within a few inches of the finished line of parquetry. The master-companion is engaged in finding the centres of his doors and windows. These centres are all marked upon the plans. He has fixed to the scaffolding outside the walls strips of deal, and tells off upon them the distance from centre to centre of the front window openings. If the walls are rectangular he tells off the same distance upon similar strips of deal fixed to the scaffolding outside the walls of the courtyard. Lines of thin cord are then stretched from one point to another, and the principal centre lines drawn as it were full size upon the works. Where these lines cut the walls he spreads upon the latter a layer of plaster; and then with the aid of the plumb line, square, and a piece of black chalk (*pierre noire*) he marks upon the plaster the centres of all openings, which marks remain until the masonry is terminated; and they serve to find the centre of the keystone in the arch over. A projecting cornice of hard stone covers the shop window lintels. The top of it forms the sills of the entresol windows, which are casements, and descend almost to the parquetry. Up to this height the stones have been lifted with slight difficulty, either rolled upon an inclined plane made of planks, or pulled up by the "*chevre*," a species of "crab." But in the meantime four tall masts, some 70 feet high, have been fixed just in front of the facade. These will be

connected at intervals with transverse planks, and further strengthened with cross pieces, the whole forming, when finished, a skeleton shaft. Within it the blocks of stone will be lifted to the height required by chains swung over a wheel fixed at the very top, the motive power being stationed at the bottom, and consisting of steam, water, or manual labor, or the last new invention—human force, however, being the most frequently employed, for in the present state of education in France it is the cheapest in the end.

The facade now being put together will be composed of solid free-stone about 22 inches thick—the blocks to be laid in the same position horizontally as they were found in the quarry. The material I am now going to lay upon the first cornice is a soft limestone, found deposited by layers and extracted in large blocks. It is not worth while to saw these blocks horizontally, even though it only requires the toothed saw to do it. The height of each course is the height of a layer of stone, for the heart of the stone ought never to be exposed to the atmosphere. The power of resistance in soft limestone to the many deteriorating influences around it depends upon the free exercise of that natural action with which it is endowed. To use it as mere ashlar facing is positive destruction to it. Soft calcareous stone easily soaks up water, but it also easily ejects it; and, provided the action be free from the outside to the inside of a stone—provided the stone be so placed as to permit the moisture it receives from the outside to be drawn away from it in a fluid state, its component parts will not suffer deterioration. But if (as in the case of ashlar where only one side is free) the exposed face of the stone dries and leaves the heart unnaturally wet, the internal moisture—which must find a way out—will crystallize upon the surface, which will become inflated and then scale off. Damp attacking a solid stone wall from the inside of the room which it encloses is drawn from it by natural means; but damp penetrating internally a brick wall faced with ashlar destroys the ashlar because the moisture cannot escape naturally through the stone, which, on its external surface, is dry. The fixing of one stone is but the repetition of another, and the laying of one floor resembles another. It may be supposed, therefore, that the front wall is ready to receive the principal cornice—the top of which is on a level with the base of the mansarde, and this level is about 65 feet 10 inches above the foot pavement, which is the extreme height allowed by law for a house on a boulevart of 20 metres and upwards. There are, however, means to construct one more story and an apology for another in the roof. The one will compose a floor of perhaps two residences and the other a host of small bedrooms, a few perhaps to be let separately, and the majority to be apportioned amongst the various tenants for the accommodation of their servants, both male and female. In the case of this house the outline of the roof must not overstep a quarter of a circle whose radius is equal to half the depth of the main building exclusive of the projecting top cornice. The chimney shafts must not break the sloping roof line except at a distance of 5 feet measured horizontally from the external face of the front wall, nor must they rise more than 2 feet above the ridge. The external face of the dormer windows must be set back at least 1 foot from the external face of the front wall; and their width must not exceed 5 feet from out to out. The compulsory setting back of the dormer windows favors the construction of a broad continuous balcony over the top cornice, which causes the fifth floor of a Paris house to be much appreciated. There is also a continuous balcony for the first floor residence, which is supported upon corbels over the entresol windows, and smaller balconies of slight projection to the other windows of this house. The balconies are constructed of slabs of hard limestone, from 8 to 10 inches thick, moulded on the face, and their soffits are paneled.

Some of the modern houses are roofed with rolled iron principal and common rafters and purlins all enveloped in plaster. I intend to eschew all battens to receive the slates and lead flashings, for it has been satisfactorily proved in France that the best foundation for lead coverings is plaster. The mode of making lead terraces, without rolls or other excrescences and presently pre-

senting a perfectly even surface, has reached a high pitch of development from the exertions of M. Monduit, who, under M. Viollet-le Duc, has successfuly revived the forgotten traditions of the lead and copper beaters' art. Strips of iron of an L-shape are to be screwed to the iron rafters, and the slates will be suspended from them upon copper hooks. The lead flashings and lead "slates," where any are necessary, will also be hooked; or they will be dressed up freely under a projecting stone fillet moulding. This practice is advisable, because the extreme heat and cold experienced at different seasons in Paris have a potent effect upon the contraction and expansion of metals. Therefore none of the lead used in my roof shall be nailed or secured with cement or mastic. The dormers shall be of beaten lead, for which the crudest possible skeleton in iron has been put together, and painted. Upon this the skilled workman adjusts the different pieces of lead—ornament, moulding, or plain covering, as the case may be. He solders them together at the back and round the iron; and the dormers, finished at the workshop, have only to be packed and sent to the building, of which they form no insignificant part. In the workshops of M. Monduit I have seen workmen converting, with hand and eye alone, rough pieces of copper into human shape, from a full size drawing suspended before them.

The roof is now covered in, the party walls are covered with stone, and the chimney-stacks, faced with stone, are finished; but the facades fronting the boulevart and the courtyard have yet to be dressed and cut into mouldings, for at present the different architraves, cornices, and pediments have been merely blocked out by the ordinary stonecutters. Another set of workmen have now to shape them according to the full size drawings of the architect. All the nearly horizontal surfaces of the stone exposed to the weather, excepting the balconies, which are of specially hard material, will be covered with zinc. As the carvers aud sculptors finish, the scaffolding descends from story to story, and the architect's troubles begin. The contractors are summoned to wait upon him at the usual hour—between 7 and 8 in the morning. An appointment is given at the works in the afternoon of the same day, when the curious passer-by will probably observe a crowd of respectable and sometimes polished-looking men standing in a forest of scaffold-poles. These are the contractors. They are seldom displeased at often being kept waiting, and some doubtless wile away the time by deploring the last revolution and discussing the next. Suddenly they all take off their hats, for the architect has arrived. As he passes through the works, followed by the whole bevy of contractors and the master companion the men salute him. From the beginning to the end of a new building he is the recognized master-of the work, whose decision is final, and whose position is unassailable. And the politeness with which they treat him is increased when, as I know from personal experience, he sometimes happens to be a foreigner.

The first thing now to be done is to fix the principal staircase. This shall consist of a comparatively new combination of iron and plaster with treads of white marble and a wrought and cast iron railing, covered with an ebony handrail. An iron carriage of an L-shape is secured at the bottom to the brick vault or to a wall. The upper edge of this carriage is cut into the requisite number of steps, and at the level of the first landing it is fixed to a joint much the same as in wood construction. Each riser is a piece of thick sheet iron fixed at one end to the carriage with an angle plate and bolted, and the other end is inserted in the wall. To form the soffit, iron bars must be placed below the risers; one end of them is secured to the flange of the carriage and the other is let into the wall. Upon these bars thin iron rods are laid, and this skeleton is filled with plaster concrete in the same way as the iron floors are treated. Not alone the soffit but the section of the stairs is thus formed. Upon the latter will be placed slabs of white marble with the moulded nosing of each returned over the iron carriage. The landings are formed with iron joists, bars, and rods enveloped in plaster concrete, and this horizontal surface will by-and-by receive a covering composed of a border of white marble filled up with small cubes of marble mosaic work. The iron standards will

be fixed to the external face of the carriage; and the soffits of the stairs and landings will be finished in plaster in the usual way. The iron portions will be painted with iron minium obtained from Belgium. The staircase fixed from top to bottom—one flight being only the repetition of another—it is possible to get up to the fifth floor residences which are in the roof. To reach the upper story in the roof, where the servants sleep and stray bedrooms are let to homeless bachelors, it is necessary to take the back staircase. The fifth floor is to be let in two comparatively small residences; all the rooms will be boarded with polished oak parquetry, and only the kitchen and the passage leading to it will be tiled. In walls of one brick or more thick, the door-frames are fixed with cramps or rather staples (*pattes*) against the face of the wall. These staples are of iron screwed at one end to the wood door post, and at the other split into claws which are inserted into the wall and fixed with plaster. On the opposite side of the wall an oak architrave, plain, and as thick as the plaster coat, is fixed in the same way against the brick. The reveal is finished with plaster. The junction of the wood and plaster where the two are flush is concealed by wood mouldings. In no part of a Paris house are there any wood lintels. In the half-brick partitions the door frames are in wood of the same thickness. In a solid stone wall the arch over a window opening is made through the whole thickness of the wall, and the rebate to receive the casement frame is cut after the building is covered in. The frame is fixed in this rebate with iron staples and plaster. In a one-brick wall the openings are covered with iron bars enveloped in plaster. The ceilings and cornices will be much more artistically finished than people are accustomed to see in England; and descending from floor to floor the ornamentation of both will increase in value. The common use of carton, and the fact that in Paris there are firms of decorators who possess in their workshops models of nearly every form of enrichment invented since the sixteenth century, renders it possible to impart to any set of rooms any fashion of architectural ornament. These applied ornaments of *carton pierre* are of a more delicate description than can be obtained in London; and celebrated decorators like the MM. Huber Brothers, are hardly known in this country; yet some of the best buildings in France, and other parts of Europe, have been carved and ornamented by them. French woodwork is often overloaded with what when painted appears to be carving, but generally the enrichment is *carton pierre* stuck upon wood mouldings. In all flat residences mirrors are placed over the fireplaces, including those of the bedrooms; and they are neither fixtures nor furniture. The best French water-closet apparatus is scientifically made, and less complicated than many patented marvels in England; but the existence of a closed cesspool renders a copious flow of water impossible: hence much inconvenience. Since the time of Louis XV. this monument of human humiliation and relief has been called by Frenchmen "*l'anglaise*"—a compliment which Englishmen with habitual modesty are probably not unwilling to accept. A host of other things ought to be described, but time is relentless. Suffice it to point out sundry differences of custom in domestic life and its architecture between Paris and London.

The ordinary Paris house is built of stone, iron, and oak; the ordinary London one of brick and deal. The common house fronts of Paris are the works of architects; scrape a superior London residence and it is Gower Street disguised. The Parisian system of iron floors is fireproof; in London it is only those buildings technically called fireproof, of which when burning the firemen are afraid. In London the gas circulates in the walls between the joists, through cellars and roofs, in pipes embedded in plaster or cement; in Paris no gas-pipes can be legally concealed except they pass freely through an iron or zinc tube, having at each end direct communication with the open air. The Parisian uses his bedroom by day, and only eats in his dining-room; the Londoner often sits all day in his dining-room and only sleeps in his bed-room. The French use carpets which are always movable, those of the English are rarely raised; of the odors which sometimes emanate from houses in London many

are born of the carpet; those of a Parisian drawing-room savor of beeswax. The walls of the latter are painted; those of even a fashionable London house are papered. Bugs are of both nationalities, but the Parisian ones find little shelter in roofs, rafters and floor-joists forming a concrete mass of iron and plaster. The Parisians use thin walls of hollow bricks, the Londoners rack their brains over the philosophy of a trussed partition. A London kitchen and its offices occupy a whole floor, and represent the extreme of wastefulness; a Parisian kitchen covers a space of about 3 or 4 yards square, and is the extreme of economy. The Parisians early every morning deposit their dust at the side of the roadway; the Londoners carefully store theirs for days together beneath their own noses. French middle-class wives help their husbands in business; English wives of the same class often ignore office or shop. The Parisians cheat posterity, the Londoners ovorburden it. The Parisians live in Paris, the majority of Londoners live out of London.

To generalize, Paris lives in a flat, London in a house of its own.

THE CHANNEL TUNNEL.

By F. C. DANVERS, C. E.

From "Quarterly Journal of Science."

It is generally accepted as a geological fact that the high mountains and deep ravines that separate divers nations, and have been adopted by them as natural territorial boundaries, were developed, in some cases by gradual changes in the surface of the globe due to natural causes, whilst in others they are attributed, apparently with good reason, to violent convulsions of Nature, by the action of which the plane of the globe has been caused to undergo great changes. However these inequalities in the earth's surface may have been brought about, the necessities of man often demand that the inconveniences caused by them to free transit should be overcome, and to this end the services of the engineer are called into request—it may be to bridge over a river or chasm, to tunnel through a lofty mountain, under a river or across a channel of the sea. Further also the engineer may be called upon to unite two seas by means of a water communication, to divert the channel of a river, or to guard the coast against the errosive force of tidal action. Taking a general view of the engineering profession, it may be broadly stated that its chief and loftiest operations are undertaken with the view of accommodating the earth's surface to the need of mankind, either by counteracting the physicial effects of forces that were in operation in ages past, or by neutralizing present active forces by opposing to them means of resistance designed and based upon a knowledge of natural laws and physical science.

Many works of past ages may fairly claim to be classed in the general list given above, of which the most celebrated in modern times, at least, will readily present themselves to the mind of the reader. There is now, however, a grand work in contemplation, for uniting England and France by means of a railway tunnel under the English Channel, which in point of boldness of design and extensiveness—both from a material and commercial point of view—cannot claim a rival.

It is generally admitted by geologists that at one time England was connected with France by land, and formed a Peninsula. To all appearances also the separation from the Continent has not been effected by any violent convulsion, but by the slow and long-continued action of the waves. It was pointed out in an article that appeared in the "Quarterly Journal of Science" for April, 1872, on the "Geology of the Straits of Dover," that, very likely, at one time when the land stood at a higher level, and before the sea had eaten out

the Straits, a river ran from South to North through the chalk escarpment, which then stretched across from Folkestone to Wissant. The higher streams of this old river are the Rother on the English side, the Wimereux and the Slack on the French side.

The probability of this country having once been a peninsula, and of the land connecting it with the Continent being washed away by the action of the sea, was carefully considered by Verstigan, so long since as 1673, in a pamphlet dedicated to King James. He compared the identity of the strata, the composition of the cliffs, the similarity of their lengths, and arrived at the opinion that the surface had been gradually worn away by the action of the sea, and not by disruption. In the following century M. Desmarest wrote an essay on the same subject, arriving at the same conclusion; and in 1818 the question was philosophically treated by Richard Phillips, F. R. S., in an elaborate essay which he read before the Geological Society.

Although, however, there appears to be little doubt that the Straits of Dover have thus been formed by the continued action of natural causes, it by no means follows that at no previous period had the even lay of the strata between England and France been disturbed by volcanic or other violent terrestrial forces. Between Folkestone and Cape Gris-Nez there is the Varne in mid-channel of a formation belonging to the Portland beds, which are of a much older series than the deposits to be found on either coast, and this of itself should prepare geologists to anticipate some irregular trend in the direction of the strata between the two shores. It may be that the irregularity is not of sufficient extent at the proposed line of passage materially to affect the projected work, but where an evident upheaval of the lower strata is known to occur within so short a distance of the selected site, it seems but reasonable to suppose that the effects of the disturbance by which it was caused may have influenced the geological formations within a few miles at least on either side of the fault. We shall, however, refer more particularly to this subject when treating of the geological examinations of the Channel bed.

It is not proposed in this article to enter into any detail regarding all the alternative schemes from time to time projected with the view of spanning more conveniently the narrow channel that now separates us from our neighbors, but it may render the present examination of the subject more interesting to refer briefly to the various devices proposed for obviating or lessening the inconveniences felt by those who suffer in crosssng between England and France from the too common complaint of *mal de mer*.

From M. Thome de Gamond's publication on this subject we learn that the establishment of some means of direct communication between England and France was first proposed at the latter end of the last century. The earliest project of which there is any account on record for crossing the Channel by a tunnel was proposed by M. Mathieu, a French mining engineer, in the service of the Department du Nord. His scheme was conceived at the close of the last century. It was laid before the First Consul in 1802, and plans illustrating the project were for several years exhibited, first at the Luxemburg Palace, then at the School of Mines, and afterwards at the Institute. Mathieu's project consisted of a subterranean way formed of two tunnels, one on the top of the other, forming in section an uneven line, the highest point being in the centre of the Channel, and inclining in opposite directions towards England and France respectively. The lower tunnel was to act as a canal to drain off any waters that might enter through leakage, and from which it would discharge at either end into drainage reservoirs. In the upper tunnel was formed a paved road, lighted by oil jets, and traversed by a service of diligences drawn by horses, which was the only known method of conveyance in those days. It is not shown exactly how the entrances to these tunnels were to be approached on either side, but they must necessarily have been situated at a great depth below the surface of the ground. For ventilating the tunnel, as well as for use in its construction, M. Mathieu proposed to erect circular iron chimneys rising above the surface of the water, and secured in position by masses of rock deposited at their bases.

When the Peace of Amiens was declared the author of this scheme thought that his project would be at once carried out. It was introduced to the notice of Fox on the occasion of his visit to Paris during the short peace that followed, and was received by him with favor: he regarded it as a most efficacious means of assuring peace between England and France. It is stated that Fox spoke on the subject to Napoleon, who exclaimed: "Oh! c'est une des grandes choses que nous pourrous faire ensemble."

Subsequently, one Dr. Payerne proposed to form a level bed at the bottom of the sea by depositing concrete, and upon this to construct a tunnel by means of diving bells. MM. Franchot and Tessier proposed to form a passage through a tube of cast-iron laid on the bed of the sea, but they appear to have suggested no means for securing a level surface. M. Favre designed a submarine tunnel having an outside casing of wood or sheet iron, and resting on piers of iron lined with brickwork in order to overcome the unevenness of the sea bed. In 1850, M. Ernest Mayer proposed a submarine tunnel between the South Foreland and Cape Gris-Nez, and some years ago, M. S. Dunn laid out a plan for constructing a tunnel under the water, of which the principal novelty consisted in a cylindrical protector, fitted with a shield in front, within which the sections of the tunnel were to be fitted together.

The most indefatigable projectors amongst our neighbors has, however, been M. Thomé de Gamond. Whilst making a geological examination of the shores of the Channel in 1833, this engineer was first struck with the idea of making a way of communication between England and the Continent, and he accordingly proceeded to make a series of soundings between Calais and Dover. M. de Gamond's first project was for the submersion of an iron tube in sections, laid at the bottom of the Straits of Dover, and lined inside with masonry. The obstacle which soon presented itself to his mind was the difficulty of leveling the bottom of the sea in order to form a bed for the tube, and this operation alone he estimated at £12,000,000 sterling, after which the cost of the tube and approaches was set down at £6,400,000; so that the total probable cost of the project was £18,400,000. This scheme was no sooner finished than it was abandoned. In 1836, M. de Gamond gave his consideration to the construction of a bridge across the Channel, taking the line from Calais to Ness Corner Point, a line shorter by about two miles and a half than that between Dover and Calais. Five different plans of bridges, in granite, in stone and metal combined, and skeleton iron structures, were elaborated by him during a period of two years. The scaffolds for commencing the works were to be supported by buoys of great size, held in their place by metallic shrouds, fixed at the bottom of the sea to strong moorings. In making the foundations for the piers, it was proposed to drive piles into the ground by manual labor, the workmen being in a water-tight chamber at the bottom of the sea. Of all the different projects for this structure, the one for a granite bridge obtained most favor amongst scientific men. This structure was designed to be 131 yards broad, the arches, 162 yards wide, were to be built on piers 52 yards long and 131 yards broad. All the arches being 57 yards high above the sea could be passed under by most vessels. There was, however, to be one movable arch to admit the passage of vessels having still loftier masts. The greatest depth of sea between these points is stated to be 197 feet, and many of the arches crossing this depth would have been 126 yards in height from the sea-bed to the key-stone. The total cost of this structure was estimated by its designer at 160 millions sterling, but by others at 200 millions. At the advice of Messrs. Stephenson, Brunel and Locke, to whom all the plans were submitted in detail, this project was soon abandoned; and in conversations which M. de Gamond had on the subject with those engineers—and especially with Mr. J. Locke—an idea was suggested of improving the means of communication between the two countries by narrowing the Straits of Dover, by throwing out piers from the two opposite shores, to be carried as far out to sea as possible, and establishing a steam communication between the two piers by means of an enormous raft moved by steam.

We defer for the present from entering further into detail regarding M.

Thomé de Gamond's more matured schemes for a Channel tunnel, whilst we refer briefly to other projects that have from time to time been put forward with the view of facilitating the Channel passage between England and France. Taking these in the order of their importance, we may perhaps devote a few lines first in considering plans that have from time to time been proposed with a view to counteract the motion of vessels in a rough sea, and so add to the convenience of travelers. The oldest invention of this sort, of which we have been able to trace any record, is mentioned in a curious old book, published in 1677, and named "Aero-Chanilos, or a Register for the Air." In it a sort of chamber is described, in which air might be rarefied or condensed, or otherwise changed for the use of invalids, so that they might have change of air without leaving home. Of this same chamber the writer says:—"Possibly, if the same might be made use of on board ship, it would (with the additional contrivance of a chair or bed, hung after the manner of a sea-compass) prevent that very troublesome affection whereto 'fresh men' are subject, called sea-sickness, and consequently become very serviceable to such whose emploiments engage them to undertake voyages into very remote parts, and there to reside far from their own countries."

The earliest patent on the subject appears to be that of Pratt (1826), in which a spring mattrass was fixed on a "swinging frame." A later invention by De Manara, in 1863, proposed to attach balloons to seats, in such a way as to keep them always horizontal. Another, by Ritchie, in 1866, describes a platform, resting on water in a tank, and having its edges attached to the edges of the tank by mackintosh, or similar fabric. Differing from all the above was a plan, patented in 1866, by M. Simpson, in which the body of the patient is firmly fastened down to the ship itself. In 1854, L. Wertheimher patented some improvements in apparatus for preventing sea-thickness, the first of which consists of a movable platform, to which chairs or couches may be attached. Connected with the platform is the piston rod of a steam cylinder, to which steam is admitted by a four-way cock, which may be opened and shut by a self-acting contrivance, so that when the ship sinks into the trough of the sea, steam is admitted beneath the piston, and the platform is caused to rise; on the contrary, when the vessel rises over the crest of the wave, steam is admitted above the piston, and the platform descends, and thus a motion opposite to that of the vessel is obtained. Another arrangement consists of three cylinders, one placed forward and two at the after part, connected with each other by pipes. The second part of the invention consists of a platform or chair, which is supported by a bracket attached to an upright shaft, which shaft passes through a hollow standard. The upper part of the shaft carries a rack in which gears a pinion, fitted with an handle, and a rising and falling motion is given to the platform by moving the handle to and fro. Or the platform may be moved by a perpendicular shaft or lever attached to a pinion gearing with a toothed rack. In the third modification, the effect is attained by interposing elastic bodies between the person and the deck.

In order to avoid sea-sickness, Mr. J. Scarth, in 1869, designed a swinging cot, which he hung from four hooks—two at each end—whilst, in order to counteract the tendency to extreme oscillation, he attached vulcanized india-rubber springs, or accumulators, below the cot, in the exact centre, directly perpendicular from the hooks by which it was hung, and this principle he proposed to apply to individual berths. In 1833, Sir J. Herschel designed a somewhat similar contrivance, which, he says, proved perfectly successful, one chief difference between his and Mr. Scarth's plan being that instead of india-rubber bands Sir John employed cord or packthread. Subsequently, in 1869, Sir John Herschel proposed, for swinging cots, to transfer the whole coercing power, operative in deadening the effects of oscillation, at once to the point of suspension; and so doing away with the necessity of attachment of any kind to the walls or floor of the cabin, whether by friction bands or by elastic straps, and this he proposed to accomplish by hanging the cot from the roof by a light but rigid iron framework, having a stiff ball-and-socket attachment lined with com-

pressed felt or other similar material, in order to offer sufficient frictional resistance to oscillation.

A floating cabin was designed by M. Alexandrovski, which, instead of being attached to a pivot, as in the Bessemer saloon, floated in a kind of tank placed amidships between the engines. This invention, it is said, was tested by the Grand Duke Constantine, in his capacity as head of the Naval Department, with a perfectly satisfactory result. All efforts to shake the cabin proving utterly unsuccessful, the pitching as well as the rolling motion of the vessel being completely counteracted. A combination of both the Bessemer and Alexandrovski plans was also recently proposed by Mr. A. Allen, of Scarborough, the object of which was to give a steady saloon cabin or gun deck at sea. This cabin was to be constructed of two spherical segments, the outer segment or dock being fixed in the ship, and the inner segment being floated on a film of water in the dock, like one basin floated in another, the inner one or cabin being maintained at its proper height by being supported on a centre pillar passing up a conical passage in its centre, and which would allow 20 degrees of roll on either side, or 40 degrees in all.

We have now to consider the several plans proposed for conveying trains across the Channel by huge ferry steamers. In the Exhibition of 1862, a proposition by Mr. Evan Leigh for conveying trains across the Straits of Dover on board large ferry boats or rafts was exhibited by means of models, but it does not appear that his project ever found any substantial supporters.

In 1865 a company was formed to place on the Channel a line of steamers from Dover to Calais, so large that the railway trains should run on board them, and there bodily remain to be run out again on the other side after crossing the Channel, and which, by their magnitude, it was expected, would ride over the waves without putting the passengers to the slightest inconvenience. The miseries hitherto inevitable to this passage had, it was remarked, been chiefly entailed by the restriction to boats of a size proportioned to the shallowness of the water on either shore. The new pier at Dover has overcome that objection on this side, and it was rumored that the French Government had granted a concession for the requisite extension of that at Calais on the other side. Such a scheme was indeed before Parliament in 1866, and was originated by Mr. J. Fowler, whose proposal was to construct steamers one-third longer than the vessels on the Kingstown and Holyhead service, and their decks were to be roofed over so as to protect the trains during the passage. The proposed dimensions of these vessels was—Length, 450 feet ; breadth, 57 feet ; with 12 feet draught of water. A Bill for effecting improved communication across the Channel by this means was three times before Parliament. In 1869 a project, having the support of Messrs. Fowler, Abernethy, and W. Wilson, contemplated the construction of very extensive harbor and dock works on either side of the Channel in addition to the construction of the large ferry steamers above referred to, and in November of that year an influential deputation from England laid the scheme before M. Gressier, the Minister of Public Works in France, by whom it was favorably entertained.

In consequence of the very defective state of the accommodation afforded by the Channel steamers plying between this country and the Continent, the Council of the Society of Arts, in 1869, offered the Gold Medal of the Society and the large Silver Medal of the Society for the best and the second best model of a steamer which would afford the most convenient shelter and accommodation to passengers on the deck of the vessel crossing the Channel between England and France. The size of the vessel was not to exceed in tonnage and draught the best vessel then in use between Folkestone and Boulogne. Seventeen models were sent in competition, which were referred to a Committee consisting of Lord Henry G. Lennox, M. P., Seymour Teulon, Rear-Admiral Ommanney, C. B., F. R. S., Admiral Ryder, E. J. Reed, C. B., Capt. Boxer, R. N., C. W. Merrifield, F. R. S., H. Cole, C. B., and Captain Tyler. From the report of this Committee it appears that three of the models only conformed to the conditions laid down by the Council, but none of these, in the opinion

of the Committee, presented sufficient novelty or merit to justify the award of the medal.

In this year also Captain Tyler, R. E., in compliance with instructions from the Board of Trade, visited the French and English coasts with a view to preliminary inquiry as to the improvements which might be effected in the means of communication between the two countries. He reported that considering the restrictions as to dimensions imposed by the circumstances of the harbors and the various conditions of the service, the steamers employed in the Channel service were admirably constructed for the work they were required to perform. With regard to Mr. Fowler's proposal for large steamers and improved harbor accommodation on both sides of the Channel, Captain Tyler observed that it was a question whether it would be worth while to ferry the railway carriages as well as the passengers across the Channel; but that the main features of an improved harbor at Dover and a new harbor south of Cape Gris-Nez were sound, if means could be found for meeting so great an expense as the works would entail. Captain Tyler then proceeded to consider the practicability of improving the existing harbors so as to fit them for a service of larger vessels, after which he directed attention to the bolder schemes which had been put forward from time to time for avoiding the use of steam vessels altogether by the construction of bridges, or tunnels, or tubes over, under, or in the bed of the Channel, with or without islands, piers, or air-shaft, so as to connect the railway system of England directly with that of the Continent.

After briefly referring to the plan proposed by M. Mathieu, Captain Tyler proceeded to observe that, after a series of geological investigations, M. Thomé de Gamond also proposed, in 1856, the construction of a tunnel, and his propositions were submitted to the examination of a scientific Committee by order of the French Emperor in that year. That commission appears to have come to the conclusion that it was desirable to test his investigations by sinking shafts and driving short headings under the sea at the expense of the two Governments. Mr. Low, an English engineer, also laid his plans for a tunnel before the Emperor in 1867, and Mr. Hawkshaw, whose attention had been for some years directed to the subject, caused a trial boring to be sunk on each side of the Channel in 1866, in order to test practically the result of his geological investigations. Mr. Remington published a plan for a tunnel in 1865, and deposited plans and sections of it with the Board of Trade. And amongst the names of other proposers or projectors in this direction may be enumerated Messrs. Franchot, Tessier, Favre, Mayer, Dunn, Austin, Sankey, Boutet, Hawkins Simpson, Boyd and Chalmers. Of these various projects those which have of late made the most progress are the bridge scheme of M. Boutet, and the tunnel scheme presented under the chairmanship of Lord Richard Grosvenor, with Messrs. Hawkshaw, Brunlees and Low, as engineers on the English side, assisted by Messrs. Talabot, Michel Chevalier, and Thomé de Gamond, on the French side. The result of the deliberations of a French commission, which was appointed by the Emperor, and presided over by M. Combes, the Director-General of the Ecole des Mines, to inquire into this last-mentioned scheme, were on the whole favorable as regards the geological and engineering parts of the project, though the members of the commission were divided as regards its financial prospects, the president and two members attaching more importance to the "utility and grandeur of the undertaking," and three other members looking at it from a more strictly economical point of view. The General Council of Pont-et-Chaussées, presided over by the Minister of Public Works, to whom the matter was afterwards referred, were unable, "upon the documents submitted to them, to decide on the probability of success of the tunnel under the Channel," and considered that "if from political considerations, the undertaking should be considered useful, the Government should follow up the investigations at their own expense."

In reporting on the different projects put forward with a view to improving the means of communication between England and France, Captain Tyler, referring to the last mentioned project, remarked:

"In this scheme it is proposed to com-

mence by driving preliminary driftways through the gray chalk, at a great depth below the bed of the Channel, between a point near Dover and another point near Calais ; as it is conceived that this material would be easily cut through, and would not be likely to present insuperable difficulties from the influx of water ; whereas Mr. Remington selects the line from Dungeness to Cape Gris-Nez, in order to avoid the chalk, and the fissures which he fears to encounter in it, and to work in the Wealden formation, which would, he believes, afford a greater chance of success.

"In the case of M. Boutet's bridge scheme, an association has been formed for making experiments, two small bridges have been built in France, and arrangements are made near St. Malo for a third, a mile in length, to be constructed in two spans of half a mile each. The Emperor Napoleon visited the works of M. Boutet, on a site granted by the French Government, and His Majesty is stated to have expressed himself favorably with regard to the project. This bridge is intended to cross from Dover to Blancnez, and is advocated, in a paper forwarded on the 27th June to the Board of Trade as (1) being less costly than a tunnel ; (2) occupying less time in construction ; (3) giving no trouble in ventilation ; (4) avoiding the danger of sudden inundations.

"Mr. Charles Boyd has forwarded to the Board of Trade a pamphlet containing his proposal for a 'marine viaduct' from Dover to Cape Gris-Nez, constructed with iron girders on 190 towers, 500 feet apart, and 500 feet above the sea, and he estimates the cost of such a bridge at £30,000,000.

"Mr. Hawkins Simpson has addressed the Board of Trade on the subject of working a submarine tunnel on a pneumatic system, which he has termed his 'Eolian system,' for which he claims cheapness, expedition, superior ventilation and greater utility.

"Mr. Alexander Vacherot has submitted to the Board of Trade a scheme on which he has several years been engaged, and which he laid before the Emperor of the French in 1856, for 'laying on the bed of the sea a tunnel made or formed of concrete, so as to form, when completed, a monolith.' He would construct it on the shore and 'draw it down to its place in sections.' And he considers that greater economy and security might thus be obtained than by the other methods that have been proposed."

After reviewing these several projects Captain Tyler, though unable to convince himself of the feasability of any bridge scheme, considered "that it might be wise to test the practicability of a tunnel scheme by means of preliminary driftways. It is probable," he said, "that, even if any of them should hereafter be carried out in practice, they could not go forward otherwise than under the supervision of, and a previous guarantee from, the two Governments ; and obvious that, as at least 10 or perhaps 15 years may elapse before they could be made available for traffic, improvements in the shape of more convenient and larger steam vessels are required in the meantime for the better performance of the service."

In the spring of 1870, Vice-Admiral Sir Edward Belcher read a paper before the Institution of Naval Architects, wherein he expressed himself favorable to a proposed ferry scheme, and to the practicability of constructing ferry steamers suitable for the service. The outbreak of war on the Continent put an end to Mr. Fowler's project ; but, in 1871, we find M. Dupuy de Lôme at the head of a similar undertaking in France. His project was for, first, the creation at Calais of a maritime station, with 16 feet 6 inches depth of water at the lowest tides, and about 30 acres in area, connected with the shore by an iron railway jetty, making a junction with a branch of the Northern Railway—and open to the sea by an entrance 260 feet wide, accessible in all weathers, and at every stage of the tide ; secondly, the construction, for crossing the Channel, of steam vessels of large dimensions and of great power, embracing all of the most important conditions of speed and comfort, and able to carry 30 passenger carriages or goods wagons. These vehicles would be placed on a double line of rails running fore and aft ; they would be shipped and unshipped by the assistance of a system of inclined planes leading to three landing stages of different heights, and alongside which the ferries would run according to the state of the

tide. A paper on this subject was read by M. Dupuy de Lôme, before the French Geological Society, in 1873. In 1871, the Society of Arts appointed a Committee to consider and report how far the existing means of crossing the Channel could be improved. In their report several modifications in existing vessels, and new boats about fifty feet longer than the existing boats, were suggested, but an opinion was expressed that no large measure of improvement could be effected in the Channel passage unless with vessels of much larger size, which would involve, in the first instance, considerable improvements in the French harbors of Calais and Boulogne, and subsequently the extension of the low-water pier at Folkestone.

Mr. Fowler again brought forward his ferry scheme in 1872, and Mr. Hawkshaw at the same time was supporting an alternative design for an improvement of the existing means of communication by the establishment of a service of vessels of considerable size, to which the existing harbors might be adapted without an excessive cost.

The bill for Mr. Fowler's project passed the House of Commons in 1872, but it was thrown out in the Lords by a very small majority. From this date comprehensive schemes for a railway ferry across the Channel appear to have been abandoned, and in their place projects were started, the one by Mr. Dicey and the other by Mr. Bessemer, for constructing steamers of special and novel design, on board of which passengers would be enabled to undertake the passage across the Channel without fear, no matter how bad sailors they might be. Both of these vessels have now been constructed, and as they have been described in former pages of this journal, it is not necessary to enter into any detailed description of them on the present occasion.

Having now made a rapid review of the general question of improved communication between England and Europe, and of the several projects that have from time to time been proposed for the purpose, it remains only to enter somewhat more fully into detail with regard to the great tunnel scheme which, to all present appearances, is about to be commenced. In doing this, we shall purposely avoid all reference to the probable traffic and commercial results of the undertaking, and shall confine our investigations to the proposed line of route, the geological features of the strata to be pierced, and the general engineering features of the work.

Two principal schemes have been proposed for a tunnel under the Channel, the one by M. Thomé de Gamond between Eastwear Bay near Folkestone, on the English side, to Cape Gris-Nez on the French coast, and the other, which has already been referred to, and with which the names of Hawkshaw, Brunless, and Low are associated, from between St. Margaret's Bay, near the South Foreland, to a point between Sangatte and Calais. The former of these, it was subsequently ascertained, would pass through a number of different beds. In mid-channel on this line there are two shoals, known as the Varne and the Ridge, which belong to the Portland formation, the same as that to which the cliffs at Cape Gris-Nez belong, and under it are the Kimmeridge Beds. These rocks dip to the north-west, and it is supposed that somewhere between Cape Gris-Nez and the Varne the Kimmeridge clay wholly disappears, whilst somewhere to the west of the Varne and nearer to the English coast the Portland beds also disappear and are overlaid by the Wealden series. Probably the Hastings beds immediately overlie these, and above them again is the Weald clay ; but where the outcrop occurs, or what is the thickness of the various beds, is not known. M. de Gamond's tunnel, therefore, as was pointed out by Mr. William Topley in an article on the "Geology of the Straits of Dover," would pass through a number of different beds ; but how many is uncertain, for it crosses the lines along which the changes above indicated must somewhere occur. There is no doubt that it would pass through all the English divisions of the Lower Greensand, for it would intersect these very near the coast ; and it cannot be supposed that the change which these must undergo takes place suddenly. Farther out in the Channel it would probably go through Weald clay, possibly it might touch the Hastings beds ; beyond this again it might intersect the Portland, and finally it would cut through the

Kimmeridge. The various beds that would be intersected by this tunnel are of very different characters; some are highly porous and some wholly impervious.

The tunnel which it is proposed to take from St. Margaret's Bay to near Sangatte will, it is supposed, go entirely through the chalk without flints. The following particulars of this project are taken from a paper on the "Channel Tunnel" read before the Society of Arts on the 18th March, 1874, by Mr. W. Hawes, F. G. S., and from a "Statement by the Committee of the Channel Tunnel Company (Limited)" published in 1874.

In 1865 Sir John Hawkshaw began his practical researches into the nature of the strata beneath the Channel, which confirmed the theories of the geologists, threw new light on the subject, and put the question of a submarine tunnel in a position to be seriously discussed and considered by the public. Before that time he had given the subject much consideration, but in that year he caused careful geological surveys and investigations to be made of the Channel, and afterwards, in conjunction with the late Mr. Brassey and Mr. George Wythes, had borings sunk on each coast. Subsequently, by means of apparatus contrived for the purpose, he examined the bottom of the channel all across in a great number of places, and raised specimens of the sea bed for examination, by which it appears to have been satisfactorily established that the actual position of the chalk across the Channel is very nearly identical with that deduced from previous inquiries, and its unbroken continuity placed almost beyond doubt. Its thickness, determined by deep borings on both sides, is proved to be above 500 feet below high-water mark, with an ample thickness of the lower or gray chalk between the bottom of the sea—which is nowhere more than 180 feet deep—and the crown of the tunnel, as well as between the bottom of the tunnel and the green sand or water-bearing strata underlying the gray chalk. The tunnel has been placed by the engineers at such a level that the depth of strata over it will nowhere be less than 200 feet, and this depth, which is desirable for security, will permit the railway approaches to be formed with not unfavorable gradients.

In the opinion of M. de Souch, Inspector-General of Mines in France, the lines selected by the engineers of the Channel Tunnel Company is the only one which presents chances of success, and is the only rational one. The Commission appointed by the late Emperor of the French to examine this project reported that there was every reason to believe that the chalk formation extends under the Channel between Dover and Calais, and that "the thickness of the gray chalk gives a certain latitude for the maintenance of the tunnel in the same direction, even where the level of the bed of the sea may be subject to some undulations; and they believe that the existence of any great fracture in the chalk is very improbable."

On the other hand, we have the opinion of so high an authority as Mr. Joseph Prestwich, Vice-President of the Geological Society, who does not entertain such sanguine views as to the suitability of the chalk stratum for the construction of the proposed tunnel, as will be seen from the following remarks extracted from a paper read by him before the Institution of Civil Engineers in December, 1873:

"The chalk formation, which everywhere in the south-east of England and the north-west of France underlies the Tertiary series, has a maximum thickness of from 1,000 feet to 1,300 feet; but, as much of it has in this area been worn away or denuded before the deposition of the Tertiary strata, its actual thickness in the district under notice varies from 300 or 400 feet to 800 or 1,000 feet. The upper beds consist of almost pure carbonate of lime, easily worn by water, and being also soft and fissured they are readily permeable. But the Lower Chalk or Chalk Marl contains so large a proportion of argillaceous matter and silica in a state of fine division that some beds pass almost into a clay, and when unbroken and compact very little water can pass through them, and then only with extreme slowness, though this will increase under pressure. But although a small bore-hole or even a shaft may often be carried through a considerable thickness of Lower Chalk and no water obtained, the occurrence of fissures is

too uncertain to render it a reliable medium over a large area. In some cases where the Lower Chalk comes to the surface, and is more broken and fissured, the quantity of water it yields is very large, as in the instance of the Tring cutting described by Robert Stephenson, where the discharge was at the rate of 1,000,000 gallons per day; or at Folkestone, where the town water supply is obtained from the Lower Chalk of the adjacent downs. On the other hand, the Chalk Marl in France and Belgium acts as an impermeable stratum in stopping the passage of water from the very permeable Upper Chalk into the underlying coal measures; and no water was found in it either at Kentish Town, Harwich, Southampton, or Calais; but the diameter of the bore-holes by which they were traversed were very small. At Calais one spring was met with at a depth of 70 feet in the Upper Chalk, and the water was brackish, showing communication with the sea. Nor must it be forgotten that wells in the Chalk under London have to be carried or bored to depths of from 10 to 300 feet before meeting with water-bearing fissures, or else headings have to be driven in search of one. Again, the escarpment of the North Downs and that of the chalk hills of Wiltshire, Oxfordshire, and Buckinghamshire, are fringed with numerous springs, which issue at their base. These springs, although thrown out generally by the Chalk Marl, are apparently not always at the top of it, but often low down in the deposit, and they constantly wear their point of issue from a higher to a lower level. Whatever the level of the spring the water of course passes through all the superincumbent portions of the Chalk Marl. This very commonly impermeable character of the Chalk Marl has given rise to the hope that it might prove compact enough for a submarine tunnel under the Channel between Cape Blancnez and the South Foreland; but when it is considered that such a work would have to face the risks arising from the lateral passage of the inland springs and from the chance fissures so common to calcareous rocks communicating with the sea, it is feared that the difficulties would prove to be of a very formidable nature. It is to be observed that in the Channel the chalk is frequently bare, besides being unprotected by any overlying strata."

The foregoing observations demand our full respect, but, on the other hand, the possibility of tunneling beneath the sea without being exposed to an irruption of sea water is shown in the submarine galleries of some mines in Cornwall, Cumberland and elsewhere. In a treatise on Mines and Mining by Mr. Price, published in 1778, he treats especially of mining under the sea, and refers particularly to the freedom of water in such works.

It is probable, and the engineers anticipate that at the shore end of the Channel Tunnel, especially in constructing the shaft through the upper strata, a considerable quantity of water will be met with, but not sufficient to prevent the execution of the work, where pumping power of any magnitude could, if necessary, be applied. It is believed, and there appear to be reasonable grounds for such belief, that as the work attains a greater depth in the chalk, and especially after the lower or gray chalk is reached, the quantity of water will diminish, and that in mid-channel it will be less than at the sides.

The geological features of this project having now been duly considered, it remains to give some particulars of the engineering nature of the work. The distance across the Channel at the point selected is about 22 miles, but as considerable approaches will be necessary on either shore, in order to reach the level of the tunnel entrance, the entire scheme will embrace about 31 miles of railway. In the first instance shafts will be sunk on each shore to the depth of 450 feet below high-water mark, and, from the bottom of these, driftways will be driven for the drainage of the works whilst in progress, and for its permanent drainage after completion. The tunnel, which will be very similar to an ordinary railway tunnel having two lines of rails, will commence 200 feet above this driftway, and will be driven at an inclination of one foot in 80 to the junction with the drainage driftway, and then at a gradient of one to 2,640 to the centre of the Straits, where the tunnel from the English shore will meet that driven exactly in the same manner from the French shore, and, being united with it,

will complete the submarine railway under the Channel. The drainage will be from the centre of the tunnel to either end.

In the execution of this work a driftway, 9 feet in diameter, will first be carried right through, and this will afterwards be enlarged to the full size of the tunnel. The problem of the execution of the tunnel in a reasonable time has been simplified by the invention of tunneling machinery, and the machine of Mr. Dickenson Brunton, which has been tried on a practical scale by the company in the lower or gray chalk, has been quite successful. The machine works like an augur boring a hole in wood. The chalk is cut off in slices, which break up and fall upon an endless band, which loads them into wagons behind the machine. The apparatus was tried by the Company at Messrs. Lee's Cement Works, Snodland, near Rochester, in the gray or lower bed of chalk, such as underlies the Channel. It made a driftway of 7 feet diameter, and it advanced at the rate of from a yard to a yard and a quarter per hour. At this rate it would only require two years to drive a driftway of 7 or 9 feet diameter from one side of the Channel to the other, a machine being started from each side. The cost of driving a heading would consist—1st, of tunneling machines, pumps, and pumping engines; 2nd, the hand labor, which would not be considerable, as the machine requires but few hands to work it; and, 3rd, interest on the capital expended during the execution of the work, which might last two years or more. Taking these three elements of expenditure into consideration and according to the calculations of experienced contractors, it has been found that the driftway could be executed for £800,000, if it required only two years to make it.

As soon as the driftway was completed the success of the undertaking would be assured. It would furnish the necessary data for an exact estimation of the cost of the whole work and the time necessary for its execution. In fact, all that would be necessary would be to enlarge the driftway to the dimensions of an ordinary railway tunnel. It has been estimated by some engineers and contractors of considerable experience that after the driftway was finished, four years' time and four millions of money would complete the work, including the junctions with the English and French railways on either shore. Sir John Hawkshaw and the engineers associated with him, however, think it prudent to double this estimate both of time and of cost, at least until the preliminary work shall have given them the necessary data for a more exact estimate of the duration and cost of the work.

Preliminary steps to test the practicability of the project are about to be put in hand without further delay, for which purpose an English and a French Company have been promoted to carry out experimental works on either side of the Channel. An Act has been passed by the British Parliament during the past Session to enable the English Company to acquire the necessary lands at St. Margaret's Bay, and it is understood that a *projet du loi* has also been passed in the French Senate to confer the necessary powers on the French Company. The works to be undertaken by these companies consist of sinking two shafts—one on either coast—about 150 yards deep, from which an ordinary mining drifting about half a mile long will be driven under the sea. This work would be a true beginning of the proposed permanent tunnel. Its cost is estimated at £160,000, of which sum it is understood the two companies will find £20,000 each; the Rothschilds of London and Paris have each undertaken to find similar amounts; the Chemin de Fer du Nord will contribute £40,000, and the London, Chatham, and Dover and South-Eastern Railways will respectively subscribe £20,000.

It may now be confidently anticipated that the commencement of this great work will not be delayed. In the foregoing account we have purposely refrained from entering into detail regarding means of ventilation and other minutiæ of construction. The progress of the work will, however, be closely watched, and we shall hope from time to time to give further particulars of its advancement in the chronicles of engineering in this journal.

TEST OF HOWARD BOILERS AT THE FAIR OF THE AMERICAN INSTITUTE.

By THERON SKEEL.

II.

A given amount of heat being developed for each lb. of fuel consumed in the furnace it is evident that one kind of boiler may be made as economical as another by simply giving to it a sufficient amount of heating surface to reduce the products of combustion when delivered into the chimney to the same temperature.

It may however be that a certain arrangement and proportion of heating and grate surface will effect the reduction of the products of combustion to the required temperature more readily than another.

The boiler then which will produce this result for the least original cost, offering the same time the best facility for inspection and repairs, occupying the least space and being equally safe and durable, is undoubtedly the best boiler.

Generally, however, no one boiler excels in all these qualities at the same time, and the purchaser must decide how much weight he will allow to each one in selecting the *best boiler for his purpose.*

The Committee on steam boilers, on the exhibition of 1871, allowed each of the first four of these qualities equal weight. The following are their marks to a scale of 10 :*

		Safety.	Dur.	Econ.	Cap.	Mean.
Root	A.....	9	9	7.09	7.3	8 098
Allen	B.....	8.5	8.5	7.07	10	8.196
Phleger	C.....	7.	6	6.99	7.7	6.672
Low	D.....	5.5	5.5	6.93	8.1	5.760
Blanchard	E.....	6.	5	7.56	5.6	6.040

It will be observed that they omitted the item of first cost entirely, preferring to leave that to the judgment of the purchaser.

It is not a fair comparison to allow the boiler to enter the above table at different rates of combustion, for any boiler may considerably increase its rate of combustion without proportionally decreasing its economy, and it is apparent that the boiler which has the highest rate of combustion has in such a comparison the advantage.

In order that the results shall be fairly comparable, each boiler should be consuming coal at exactly the same rate during the experiments. As it is impracticable to have these conditions it is necessary to reduce them to the same rate. In doing this, they may be rated either by their heating surface or their grate surface. It appears that for this *class* of boilers, the heating surface being the measure of the first cost, should be the unit of comparison. After reducing the economy and capacity from those found in the experiment, to those that would obtain at a given rate of combustion per square foot of heating surface, the results are fairly comparable.

This method can only be assumed when there is no difficulty in burning on the grate furnished the necessary amount of coal.

The following reduction is made by the formulæ given by Prof. Rankine, on the supposition that each boiler shall burn ½ lb. of combustible per hour per square foot of heating surface. The *lbs.* of combustible per square foot per hour will be :

* Rep. Committee on Steam Boilers, Am. Inst. Fair, 1871.

Root	A	16¼	Root	A	.670
Allen	B	14¼	Allen	B	.696
Phleger	C	13	Phleger	C	.649
Low	D	12.1	Low	D	.642
Blanchard	E	20.9	Blanchard	E	.646

In reducing the economy from the experiment the ratio of all heating surface to the grate surface is used.

The following will be the economy:

As these boilers are all supposed to be burning coal at the same rate, their capacities will be in the same ratio as their economies, and the final results will be:

		Safety.	Dur.	Cap.	Econ.	Mean.
Root	A	9	9	6.70	6.70	7.85
Allen	B	8.5	8.5	6.96	6.96	7.73
Phleger	C	7.	6.	6.49	6.49	6.50
Low	D	5 5	5.5	6.42	6.42	5.96
Blanchard	E	6.	5.	6.46	6.46	5.98

It will be observed that the rate of combustion is not more than could have been obtained with good chimney draft in any case except (E).

The superiority of the (B) over the (A) in economy was undoubtedly owing to the feed water heater in the chimney, being 30 □′ of tube surface, for although there was in the experiment 40 per cent. less proportion of heating surface to coal consumed, the temperature of the gases leaving the boiler was 71° less in the "B" than in the "A" corresponding to about 3 per cent. The A had no feed water heater.

A boiler of the proportions used in the case of the (D), with the ordinary arrangement of furnace, should have given an efficiency of about 75 per cent. at the rate of combustion in the table.*

The loss of efficiency (75—64=) 11 per cent. was probably due to the admission of cold air into the combustion chamber through an opening designed for that purpose.† For it has been experimentally proved that such an admission is detrimental.‡ It will be observed that the high economy of the (C) boiler at the low rate of combustion at which it was tried, disappears when it is reduced to a rate of combustion corresponding to its heating surface. This is to be expected, for the draught was in that case forced by a blower, and the steam used deducted from the evaporation of the boiler.

Thus this boiler expended a certain amount of steam to obtain the draught which the others got free of cost from the small specific gravity of the products of combustion in the smoke stack, due to the high temperature at which they were necessarily rejected; it must be noted that the temperature in the flue of the (C) boiler would become much higher at the higher rate of combustion.

The total weight of coal fed into the furnace of the Howard Boiler, during the experiment, was 5,100 lbs., and the weight of ashes and coal drawn out was 1,158 lbs., and therefore the combustible consumed was 3,942 lbs. The duration of the experiment was 21½ hours, and therefore the lbs. of combustible consumed per hour were (3942 ÷21.5=) 183.5, and the lbs. of combustible per square foot per hour were (183.5÷27=) 6.8. The amount of refuse withdrawn from the furnace is not a measure of the amount of refuse in the coal, as a large part of the matter withdrawn was coal which might have been consumed if the experiment had continued longer, and if the refuse had been picked.

If the proportion of refuse in the coal

* Report of Board on Vertical and Horizontal Tube Boilers. 1868.
† Report of Board on Trial of Steam Boiler, Am. Inst. Fair, 1871.
‡ Ex. Res. in Steam Engineering, Vol. II., p. 58, Vol. I., p. 275.

were one-sixth of its weight, an average amount for anthracite coal, the total weight of *coal consumed* would have been (3942+3942÷5=) 4730; and the weight of coal per hour (4730÷21.5)220; and the lbs. of coal per hour per square foot = (220÷27=) 8.15. This is a very low rate of combustion. In ordinary practice, such a boiler should have given a rate of from 18 to 22 lbs. of coal per square foot per hour, where the fires were urged as in this case.

The cause of the slow combustion was undoubtedly a leakage of air into the chimney through the boiler casing.

This cold air acted to reduce the combustion in two ways:

1st. By reducing the temperature of the air in the chimney, and thus reducing the velocity of the ascending current, and the volume of air delivered.

2d. By supplying a portion of the air delivered by the chimney from the outside, and thus still further reducing the amount which must pass through the fire.

The effect of the reduction of temperature on the draught is small, for while the velocity of the ascending current is *reduced* its density is *increased*. The effect of these two simultaneous changes nearly balance, although there is a gradual increase of the weight of air developed up to about 600°.

The ratio of the weight of air, which was delivered by the chimney to that which would have been delivered if the temperature of the air in the chimney had been the same as that leaving the boiler, *i. e.* 328° may be completed from the formulæ applicable to be as 100:108.*

Thus, if the air in the flue had not been diluted after leaving the boiler, but had had in the chimney a temperatare of 328° in place of 234°.5, the furnace would have consumed (183.5 × 1.08=) 198.2 lbs. of combustible per hour, being (198.2÷27=) 7.3 lbs. per square foot.

The temperature of the gas in the chimney was reduced after leaving the boiler by the addition of a certain quantity of air which leaked in through boiler No. 4, and past the dividing wall as before described. The portion of air at

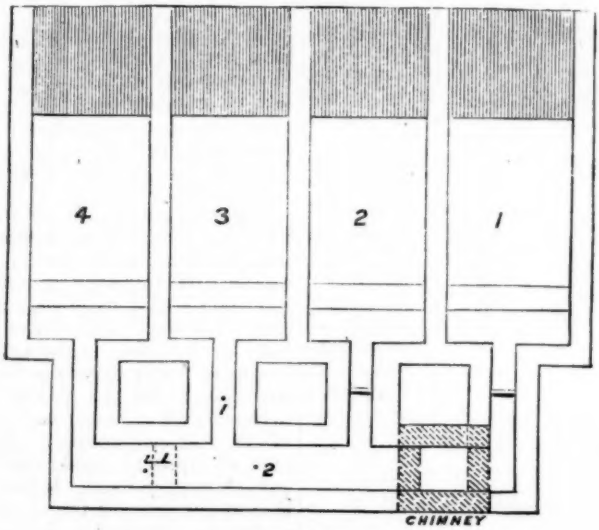


FIG. No. 4.

a temperature of 45°, which would by mixing with a unit of gas at 328° give a resulting temperature of 234°.5, is found to be $(100 \times \frac{328-230}{230-45} =)$ 53 per cent. Of all the gas passing up the

* Steam Engine and other Prime Movers. Rankine, p. 288. Ed. 1866.

chimney then ($100 \times 53 \div 153 =$) 65.4 per cent. passed through the No. 3. If the leakage of air through Boiler No. 4 had been stopped, the same head of gas in the chimney would have drawn more air through the coal in the ratio [$100 : \sqrt{(100)^2 + (53)^2} =$] 113.5, and therefore the rate of combustion would have been ($198.2 \times 113.5 =$) 225 lbs. combustible per hour, and ($225 \div 27 =$) 8.3 lbs. of combustible, or $\left(8.3 + \frac{8.3}{5} = \right)$ 10 lbs. coal per square foot of grate per hour.

The combustion was also reduced by choking of the connecting flue between the boiler, No. 3, and the main flue by the sand pot and bricks, as shown in Fig. No. 8.

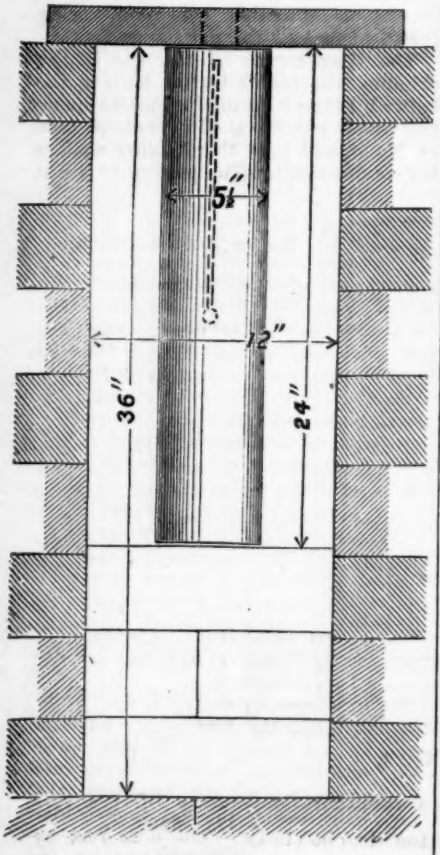


FIG. No. 8.

The original section was 3 square feet, being one-ninth of the grate surface, and the reduced section was 1½ square feet, being 1 : 21.6 of grate surface.

It will be observed that there is another section, being the 1st connection of but little more than this, but as that was only reduced so small by a small excess of length of the iron plates laid upon the tubes, which could be easily remedied, it may be disregarded.

It appears from comparison of other boilers that for any cross sections of flue less than ⅛ the grate, the amounts of coal that may be burned are in the ratio of the square roots of the ratio of the sections. Thus, if the section of the flue had been ⅛ of the grate in place of $\frac{1}{21.6}$, there would have been more coal consumed in the ratio ($\sqrt{21.6 \div 9} =$)1.55 : 1, whence the consumption of combustible per hour would have been ($225 \times 1.55 =$) 310 lbs., and the lbs. per square foot of grate per hour = 11.5 lbs., and the lbs. of coal per square ft. per hour ($11.5 \times 1.2 =$) 13.8. Therefore, the most rapid combustion possible in the boiler, as built if it had been connected to the chimney independent of the other boilers, would have been 13⅗ lbs. of coal per hour. The economy would have been* at this rate of combustion 9⅘⅝ lbs. of water from 212° per lb. of combustible, and the capacity 2,964 lbs. of steam per hour from 212° temperature of feed water.

It is believed that the rate of combustion was injured by the air which leaked into the combustion chamber through the false front to the boiler. This front was formed of cast iron plates not planed, and only held loosely together by lugs cast on them. The plates exposed a joint 70 feet long in the aggregate. It has been found experimentally that air will flow under the pressure of the experiment, $\frac{1}{100}$ inches of water, at the rate of 18 feet per second.† Each square inch of opening would then deliver ⅛ cubic feet per second. It has been found that 53 per cent. of the air passing from boiler No. 3 leaked in through the false front of boiler No. 4. If the same amount should have leaked in through

* Steam Engine and other Prime Movers. Rankine. Ed. 1866.

† Experiments on flow of air. Wilson Engineering, 1872.

boiler No. 3's false front, and there would appear to be no reason why this is not probable, only (100−53=) 47 per cent. of the air passing through the tubes passed through the fire. If this leakage through the false front should be stopped, the same head of gas in the chimney would consume more coal in the grate in the ratio of $\sqrt{100}:\sqrt{37}=100:61$, and the maximum combustion would be (310×100÷61=) 510 lbs. combustible per hour, being $18\frac{6}{10}$ lbs. combustible, or 22 lbs. of coal per square foot per hour.

It is believed that is the maximum for this boiler.

The quality of the coal is always a vexed question on comparison of experiments on boilers. It is believed that this may be computed with a sufficient degree of accuracy from the measurements recorded in this experiment.

The weight of water evaporated, and that entrained with the steam measure, the amount of heat passing through the metal of the boiler into the water. The weight of gas passing up the chimney with its elevation of temperature measures the amount of heat passing up the flue.

If there is added to these the heat lost by radiation, and that remaining undeveloped in the gas owing to incomplete combustion, we have the total heat contained in the material consumed. The method of measuring the water evaporated and entrained, has been explained.

The volume of gas passing up the chimney was computed by multiplying its velocity by the mean area of the flue. The velocity was measured as follows:

A small hole one inch in diameter was drilled through the side of the chimney, 50 feet down from the top. A small piece of cotton wool was placed in this hole, where it laid held by the friction of the walls. An observer was stationed at a point in the yard, from which both the top of the chimney and the opening through the side were visible. At a given signal, the wool was crowded through the side of the chimney with an iron rod. The observer in the yard noted the interval between the times when he saw the wool crowded through the side of the chimney and when it appeared at the top.

The mean of twelve experiments gave an interval of 8 seconds, whence the velocity of the wool upward in the flue was $\frac{50}{8}=6.25$ feet per second. The velocity of specimens of the same material was afterwards found to be, when falling in still air, 5.15 feet per second, and therefore the velocity of the gas in the flue was (5.15 + 6.25 =) 11.4 feet per second.

A part of the chimney, for 30 feet above the opening, had an area of 4.0 square feet and the balance, the upper 20 feet, an area of 4.9 square feet, whence the mean area was (4×30+4.9×20)÷50=4.36 □′, and the volume of gas delivered was, per hour (4.36×11.4×3600 =) 178920 cubic feet.

The weight of a cubic foot of air at a temperature of 234.6, the mean temperature of the chimney, was .0572 lbs., whence the weight of air passing up the chimney in lbs. is(178920×.0572=)10234 per hour. In addition to this amount of air, there were 183.5 lbs. of combustible matter from the coal also passing up the chimney per hour, making the weight of gas (10234 + 183.5 =) 10417.5. This weight of gas was raised from a temperature of 35° to 235°, and if its specific heat were $\frac{.24}{100}$, the units of heat carried away by the gas would be per hour 10417.5×200×.24=500016, and for each lb. of combustible (500016 ÷ 183.5=) 2724. The heat equivalent to the steam produced by one lb. of combustible is (10.53×966=) 10171, and equivalent to the water entrained is (.823×294°=) 242. The portion of the heat dissipated in the three ways mentioned then is for each lb. of combustible:

Steam	10171
Water	242
Gas	2724
	13137

There must be added to this the heat dissipated by radiation, and that not developed by combustion.

The heat lost by radiation was probably small, but the Committee have no means of estimating it. It was, however, not sufficient to sensibly elevate the temperature of the fire-room, the room being during the day time colder than the outside air. The heat lost by incomplete combustion may be taken at,

at least six per cent., as has been found in similar boilers and rates of combustion when the *holes in the furnace door were closed*,* whence the thermal equivalent becomes (13137×106=) 13925 units of heat.

Probably the thermal equivalent of this coal may be taken to be 14,000 units of heat per lb. of combustible, the combustible being understood to be the material of any kind whatever which disappears in the furnace. One pound of combustible will be capable of evaporating 14½ lbs. of water at and from 212°.

One probable reason why the combustible matter showed less heating powers than the average coal is that only a portion, and that the least efficient portion, of the combustible in the coal was consumed.

Thus, there was supplied to the furnace 5,100 lbs. of coal, whose probable composition was:

Carbon,	89.8 per cent...	4,577 lbs.
Volatile,	5.5 per cent...	281 lbs.
Ash,	4.7 per cent...	240 lbs.

There were withdrawn from the furnace at the end 1,158 lbs., and consumed 3,942 lbs.

Probably all the volatile products were expelled, as the whole material was heated above a red heat, and therefore the carbon consumed was only (3942—281=) 3661 lbs., being (3661÷3942=) 92.8 per cent. of the combustible consumed in place of 95 per cent., as in the combustible in the coal. The coal burned during the experiment gave evidence of a considerable gaseous component.

In place of a glowing mass of white hot coals, as is the usual appearance of anthracite, the whole fire-box and space was filled with a mass of yellow flame. It is supposed that a part of this flame was the result of an insufficient mixture, not an insufficient supply, of air, and that this mixture would have been aided if the holes provided in the furnace doors had been left open.

These holes were, contrary to the advice of the Committee, kept closed during the whole experiment.†

* Ex. Researches on Steam Engineering, Vol. I., page 215.

† Ex. Res. Steam Engineering, Vol. I., page 146.

At about 10.30 P. M., while the fire box was filled with yellow flame, a bucket of water was thrown on the hot ashes in the ash pit under the grate. This water was instantly transferred into a cloud of steam, which passed through the coal into the furnace with considerable force.

The entrance of the steam was immediately followed by a loud explosion in the furnace of sufficient violence to force open the door, which was closed but not fastened, and to drive out a mass of flame a yard long through the furnace door, and *through the fissures in the front* into the fire room.

The manner of connecting the lower rows of tubes of the boiler left openings between the heads, these openings being fissures 12″ to 14″ long, and of sufficient width to see through into the furnace. These fissures must have allowed a considerable volume of air to leak through into the nest of pipes. If this same quantity has been admitted into the furnace where the gases were sufficiently hot to unite with the oxygen of the air in the same manner, it would probably have aided combustion, but being admitted after they had lost a large part of their original temperature by passage through one row of tubes containing water at 35°, the effect was probably injurious to both the economic and potential effect.

It appears from inspection of the foregoing that about 36 per cent. of the air passing up the chimney passed through the fire, being (10234×.36=) 3684 lbs. per hour, and (3684÷183.5=) 20.75 lbs. of air per lb. of combustible consumed.

This is probably (20.75÷11.5=) 1.98 times the air necessary for perfect combustion, as has been found in many other cases.

This coincidence is cited, not to prove the quantity of air passed through the coal, but as the quantity of air passing through the coal, being the balance after deducting the other losses, appear to be what it is well understood it must have been, to prove the correctness of the computation of the *volume* of *air* and the *leakage*.

The amount of air leaking in through the front must have mixed with the products of combustion soon after passing through the three lower rows of tubes,

and must have reduced the temperature of the products of combustion to the temperature at the flue 328° *at once*. It would appear from this that the boiler would have been more efficient if the three upper rows of tubes had been left off, for they only acted to condense a portion of the steam which had vaporized in the lower tubes. This would not be true if the front were tight.

It will be observed that the temperature of the gas leaving the boiler is less than the temperature of the steam due to the pressure. Under these circumstances, no superheating is possible with any superheater whatever. It will also be observed that at one time the temperature of the gas leaving the boiler is considerably hotter than the temperature due to the pressure of the steam, but the temperature indicated by the thermometer on the steam drum is less than the equivalent temperature of the steam. The following table exhibits this variation :

Hour.	Pressure.	Measure Temperature.	Equivalent Temperature.	Temperature of gas.	Differences.	
					Gas and Steam.	∞ Temp. and Meas. Temp.
9.30	72	319	317.3	350	+32.7	+ 7.4
10.00	73	311.5	318.2	375	57.8	− 2
10.30	78	311.5	328.2	400	77.8	− 6.7
11.00	74	311.5	319.	415	94.0	−16.7
11.30	76	310.5	320 6	390	69.4	− 7.5
12.00	75	310	319.8	390	70.2	−10.1
12 30	80	316	323.6	380	56.4	− 9.8
1.00	70	320	323.6	350	26.4	
1.30	77	311	321.4	330	8.6(+57.8)	3.6(−8.)
2.00	80	319	323.6	310	−22.6	−10.4
2.30	70	307	315.8	320	+ 4 2	− 4.6
3.00	75	312	319.8	315	− 4.8	− 8.8
3.30	79	318	322.9	310	−12.9	− 7.8
4.00	75	312	319.8	310	− 9.8	− 4.9
4.30	84	318	326.6	315	+11.6	− 7.8
5.00	73	310.5	318.5	315	− 3.5	− 8 6
5.30	79	315	322.9	310	−12.9−(10)	8.0(7.64)

An inspection of the above table shows that when the gas was leaving the boiler 57½° hotter than the temperature of the saturated steam, the thermometer in the steam pipe indicated 8⅛ degrees less than that temperature, and when the steam was leaving 10° colder than the temperature of the saturated steam, the thermometer on the steam pipe indicated only 7⅛° less than the temperature of the steam. The inference to be drawn from this is that the steam was not *superheated in either case*.

Your Committee do not expect that it can be considerably superheated in any case, because from an inspection of the boiler, it will appear that all the steam formed will pass out through the back end of the lower tubes, and pass directly into the steam drum without entering the superheating tubes at all, and that they will probably be filled with a stagnant mass of steam.

This course of reasoning applies to all boilers of this class which have the steam drum connected with the high end of the inclined tubes. By simply moving the connections and the steam drum to the other end, the steam could be made to pass through the superheating tubes, *en route*, and they would form very efficient superheaters.

Notwithstanding the considerable amount of superheating or steam drying surface, there was a considerable amount of water entrained with the steam.

Your Committee cannot say how much more or less would have been so entrained if the fires had been forced by a stronger draught, so that the gases

would have left the boiler hotter, but it appears probable that there would have been more if the combustion had been more rapid, for during the first part of the first experiment, when the fires were new and thin and burning rapidly, and when the temperature of the gas leaving the boiler was many degrees hotter than the steam, there was nearly 10 per cent. of the water entrained, while the average for the whole experiment was only $2\frac{5}{10}$ per cent.

It will be observed that the per centage of priming is less during the first experiment when the steam pressure is lowest. This is contrary to the generally preconceived idea, and your Committee do not regard this experiment as a final proof of this fact, but have no doubt that the priming was really greater in the case with the higher pressure, for it is shown by the increased apparent evaporation per lb. of combustible, as well as by the measurements in the still.

The diagram of steam pressures, taken by the Edson Time Gauge, a copy of which is annexed to this report, shows that the pressure varied from $8\frac{1}{2}$ lbs. above to $6\frac{1}{2}$ lbs. below the mean during the 1st experiment, and from $7\frac{5}{10}$ above to $8\frac{3}{10}$ below during the second experiment, be a mean variation of $7\frac{6}{10}$, and the air extreme variation of $15\frac{1}{2}$ lbs.

It must be remembered that this variation was, while the feed and volume of steam discharged through the safety valve were regulated to keep the steam as nearly uniform as possible.

The same variations show in the boilers of this class, tested in 1871.* If there had been a constant volume of steam drawn from the boiler, as would have been the case while doing any useful work in a factory, the variation would have been more.

At the conclusion of the experiment, with the steam pressure at 75 lbs., the safety valve was closed, and the steam allowed to accumulate in the boiler until the pressure reached 135 lbs.

During this interval the fires were burning at the same rate as during the other parts of the experiment.

The following table shows the increase of pressure :

Time.	Steam.	Time.	Steam.
hrs. min.		hrs. min.	
5 30	79	5 49	115
5 43	90	5 50	118
5 44	93	5 51	122
5 45	100	5 52	125
5 46	103	5 53	131
5 47	105	5 54	135
5 48	110	5 55	blow off.

The boiler had been fed with cold water just before 5.30, and it is believed that the temperature of the water in the boiler was not uniform until after 5.43, after which the increments of temperature appear to be nearly uniform, being $2\frac{5}{10}$ degrees per minute.

The weight of water in the boiler was 4,400 lbs., and the equivalent weight of iron surrounding it was 1,200 lbs., giving the total equivalent weight of water to be elevated in temperature to be 5,600 lbs. The increase of the temperature of the water was from 5.43 to 5.54, being 11 min., (358.45—331.18=) 27°.25. The units of heat corresponding to this elevation of the temperature of the whole mass would be (27.25×5600=) 178300.

This amount of heat was transferred in 11 minutes. The amount transferred during one hour, at this rate, would have been (178300÷11×60=) 861800 units corresponding to (861800÷1156=) 741 lbs. of steam from the temperature of the feed.

But during the interval immediately surrounding the elevation of temperature, the boiler was evaporating (183.5 ×8.8=) 1615 lbs. of steam per hour. If this rate of transfer had continued during the elevation of temperature the increment would have been (2.1×1615÷ 741=) 4°.4 per minute. So far as this experiment goes, although your Committee do not consider it conclusive, it would appear that the heat was only transferred about one half as fast while the pressure was rising as while it was uniform.*

If the heat should be transferred during the whole time while the pressure was rising to that pressure at which the boiler would burst, at the same rate as during the elevation of pressure between the two experiments, the bursting press-

* Report of Committee on Steam Boilers, 1871.

* Heat and Steam Engine. Trowbridge, page 106.

ure would be reached in nearly 23 minutes; for the bursting pressure of the tube is 3,000 lbs., and the temperature corresponding to 3,000 lbs. in 797°, and (797−320÷2.1=) $22\frac{3}{4}$ minutes. This is about half as long a period as would usually be occupied by the ordinary forms of boiler. The rapid variation of pressure is the result of the small weight of water contained in the boiler. This is an important feature of all the boilers of this class to which this belongs. It must be remembered that the cast iron heads will probably give out a less pressure than 3,000 lbs., although your Committee have no means of determining at what pressure they will give out.

A failure of one of the heads will probably relieve the steam pressure, without causing an explosion in the ordinary acceptance.

The usual way in which boilers wear out is by the burning of the heating surface, where it is covered by a deposit of scale on the water side, or rusting through in places where it is exposed to the air and moisture, as when the iron touches the brick wall, or where any leakage may allow the tube to be continually wet.

In cases where the feed water contains any salts, which are deposited by concentration, or by elevation of temperature scale, will form in all boilers alike, regardless of form or size. The only advantage which one boiler can have over another in this particular, is the facility which it may offer for breaking off this scale, after it has reached a sufficient thickness, to injure the heat transmitting power of the plate. In ordinary tubular boilers, this cleaning can scarcely be done at all, and in flue boilers can only be done very ineffectually, even if the flues are of a large size.

In the Howard Boiler, the whole inside surface of the tubes may be exposed to view by simply removing the caps. A layer of scale of any thickness can easily be removed by driving through the tube a tool with a number of longitudinal grooves, and then breaking down the intermediate portions with a bar. The only portions which cannot be cleaned are the heads, which are but a small part of the heating surface.

SAFETY.

The strength of the tubes of this boiler being $8\frac{3}{8}$″ inside diameter, and $\frac{5}{16}$″ thick, taking the strength of the iron at 50,000 lbs., and the welded joint at 20 per cent. less will be $\left(\frac{.80\times 50{,}000}{8\frac{3}{8}}\times\frac{5}{8}=\right)$ 3,000 lbs. nearly per square inch. The cast iron heads are probably not so strong as the tubes, but the Committee have no means of determining how strong they are.

If it is true, as said by an eminent engineer, that "in nine cases out of ten a continuously increasing pressure of steam without means of escape, is the immediate cause of explosion."* The liability to explosion would decrease as the margin of strength increases, and this must be looked upon as a very strong and safe boiler.

There is another point, however, which the Committee do not feel justified in passing without comment. The feed water is introduced into all the lower rows of tubes from a common pipe, and these tubes are only connected with the steam space by the back end. Now, it is evident that if there is any steam formed in the lower tubes, it must leave them by the back end, and that there would be a constant current of steam leaving the tubes, tending to carry the water with it. It is no doubt the expectation of the builder that there will not be any steam formed in the lower tubes, but that they will only serve as a feed water heater, and that the cold water pumped in at the lower end will be gradually forced along the tube by the fresh supply of water coming on behind it, becoming warmer and warmer as it travels along the tube, but not reaching the temperature of the steam until it arrives at the end and mixes with the water circulating through the upper tubes. So long as this is the case, no harm can come from having *these* tubes connected *at one* end only. The effective surface of the lower row of tubes being one half the total surface, 50 square feet is equal to ($\frac{4}{27}$=) 1.85 time the grate surface. From experiments with other boilers, at the same rate of combustion, it appears that about 6,000 units of heat would be absorbed by this surface for every pound of combustible consumed. During the experiment, the feed water

* Sir William Fairbairn—Useful Information for Engineers. 1st Series, p. 58. Ed. 1864.

had a temperature of 35°, and each lb. of combustible elevated 9 lbs. of water from the temperature of the feed to the steam, and evaporated 8.8 lbs., the balance, 2 lbs., being entrained with the steam. Thus the total heat absorbed was :

Water	$9 \times 286 = 2574$
Steam..	$8.8 \times 888 = 7814$
	10388

Therefore the units of heat available to form steam in the lower tube (6000—2574=) 3426, and the equivalent weight of steam (3426÷888=) 3.86 That is, (3.86÷8.8=) 44 per cent. of the total steam formed in the boiler will be formed in the lower tubes. If the water received heat uniformly from the time of its entrance during its passage along the tube, it would have acquired the temperature of the steam at (2791÷6000×12=) 5.6 feet from the front end of the tube at this point the steam would commence to form and the current of water would be reversed.

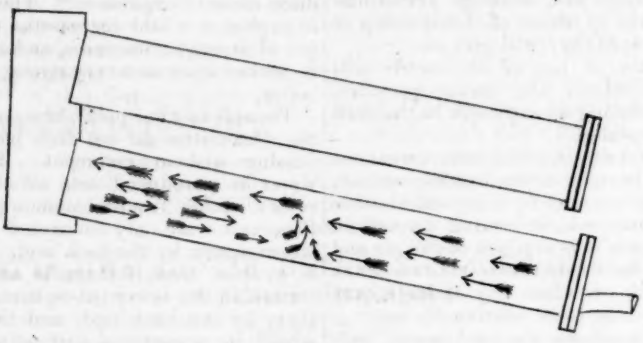


FIG. 14.

If the combustion is slow, as in the experiment, these two currents may pass each other without interference, but if the combustion be sufficiently rapid so that more heat is thrown on the lower tubes, or if the feed water enters the tube at a higher temperature than 35° so much steam may be formed that the opposing currents of steam and water will interfere, and drive the water from the tube when the tube would soon become red hot, and burst with a very moderate pressure. Although it is not probable that there would be an explosion in the ordinary acceptation of the term, still the steam and water would pour out into the furnace with certainly disastrous and perhaps fatal results. Your Committee cannot say at what rate of combustion or temperature of feed, steam would be formed in the lower tubes with sufficient rapidity to expel the water. That can only be determined by experiment, but they refer to the performance of a similar boiler, the boilers of the steam ship "Montana," which would seem to corroborate these views.*

In the form of boiler, at present manufactured by the exhibitors the tubes are connected at *both ends* which would obviate this trouble.

If there were consumed in this boiler ½ lb. of combustible, for every square foot of heating surface per hour, the percentage of perfect absorption would be, computed as in the other cases, 62.4 per cent.

In comparing this boiler experiment with those made under atmospheric pressure, and the ordinary temperature (60°), an allowance should be made for the greater proportion of the heat *necessarily* rejected through the chimney, for the gas must at best be discharged at the temperature of the steam, if there is no feed water heater.

This allowance will be for the Howard boiler under these circumstances of the trial, [(335—35)÷(212—60)=] nearly 2; the heat necessarily wasted in the ordi-

* Nautical Magazine, London, November, 1873.

nary atmospheric test, which is [(212—60) ÷(2240—60)=] 6.8 per cent., and therefore in the Howard boiler (2×6.8=)13.6 per cent.

That is, if the Howard boiler had been tried under atmospheric pressure, and with an atmospheric temperature of 60°, the per centage of heat usefully absorbed would have increased from 62.4 to (62.4+6.8=) 69.2.

The same reasoning applies to the boilers tested at the Fair, in 1871.

Safety, facility for repairs and durability, are the three most important points of excellence in a type of boiler, for with proper proportions of calorimeter and heating surface all types becomes equally economical.

Your Committee consider that, under ordinary circumstances, this boiler is equal in safety and economy, and is superior, under all circumstances, in durability and facility for repairs to any boiler of its class with which they are acquainted, and they therefore recommend you to award it a *silver medal.*

Your Committee have endeavored, so far as they were able, to anticipate the action of this boiler, and the reasons for its peculiar construction, from an examination of the few sections exposed at the exhibition, but have received in this no assistance from the exhibitors.

Respectfully submitted by the Committee,

R. H. Thurston,
Thos. J. Sloan,
Theron Skeel.

To the Board of Managers of the American Institute Fair.

ON THE CONDITIONS OF WATER BEFORE AND AFTER ITS FREEZING POINT.*

From "Engineering."

Apropos of the question which lately arose from the report of Mr. Lavington E. Fletcher, engineer to the Manchester Steam Users' Association, as to the effects produced upon a boiler filled with water and ultimately frozen, we think that a few remarks upon the condition of water before and after its freezing point, may, to a certain extent, harmonize the diversity of opinion on the subject, which evidently now exists. In so doing we do not pretend to offer any new laws or theories, but by placing before our readers in as concise a manner as the subject warrants, the results of the experiments of such men as Kopp, Despretz, Pierre, Gineau, Regnault, and others, we hope to be able to assist in the solution of a question, possessing unusual interest and importance to the engineer and the physicist.

Although observation and experience, aided by sound inductive reasoning, are the truest guides in experimental researches, we should do well to remember in our investigations the following extracts from Sir Isaac Newton's celebrated *Reguli Philosophandi.* "We are to admit no more causes of natural things than such as are true, and sufficient to explain their appearances." "In experimental philosophy, we are to look upon propositions collected by general induction from general phenomena as accurately, or very nearly true, notwithstanding any contrary hypotheses that may be imagined; till such times as other phenomena occur, by which they may be made either more accurate or liable to exceptions."

All liquids, to a certain extent, expand by the application of heat, and this fact at once shows the fallacy of the commonly accepted notion, that water is incompressible; for the dilation and contraction of the liquid is simply extension and compression of its particles. This subject has been fully investigated in Germany by Kopp, and in France by Pierre; many very valuable and important facts being discovered by their careful investigations. The apparatus used by them was nearly similar, and consisted simply of a water thermometer,

* The Centigrade scale is used throughout.

graduated to the different degrees of expansion; corrections being made in all cases for the expansion of the glass. To determine the compressibility of water still more accurately, Regnault designed a special apparatus, and with it Grassi determined the following results:

Pressure in Atmospheres.	Temperature.		Compression, expressed in Fractions of Original Volume.
	deg. C.	deg. F.	
1	0.0	32	0.0000503
1	4.1	32.38	0.0000499
1	13.4	56.1	0.0000477
1	25.0	77.0	0.0000456

He also found that the compression or condensation of water was proportional to the pressure.

Although the expansion of water is comparatively small between its boiling and freezing points, yet it is the most irregular of all liquids; so irregular in fact, that it has been found impossible to find a single empirical formula to express the expansion at different temperatures. Below 10 deg. C. (50 deg. Fahr.) it is more irregular than above that point, for water possesses what no other liquid has been discovered to have, and that is a point of maximum density. This singular anomaly is so remarkable, and its consequences so important, that we think it advisable to dwell upon it for a short time.

If we take a water thermometer and expose it to the cold, we shall observe the following curious phenomenon. The liquid will gradually descend until it reaches the temperature of 4 deg. C. (39.2 deg. Fahr.); at this point the contraction will cease, and although the cold acting on the bulb is far below this point, the liquid will gradually ascend, until it reaches 0 deg. C. (32 deg. Fahr.), or freezing point, when it will solidify. The point at which the liquid commences to ascend is called its "point of maximum density." An interesting experiment to illustrate this fact, was designed by Dr. Hope, and is as follows. Take a tall jar, and fill it with water, say at 15 deg. C. (60 deg. Fahr.); at the top of the water fix a small mercurial thermometer, and another one at the bottom; then place the jar at rest, exposed to the cold. The bottom thermometer will be observed to fall more rapidly than the top one until it reaches 4 deg. C. (39.2 deg. Fahr.), when it will remain stationary. The top thermometer will now fall, and continue to do so, until the water freezes; the bottom thermometer still remaining at 4 deg. C. (39.2 deg. Fahr.), These effects are easily explained; the particles of water at the top being exposed to the cold, decrease in temperature, thus becoming denser and fall to the bottom, their places being taken up by warmer particles, which in their turn undergo the same change, until the whole volume has completely circulated and attained a temperature of 4 deg. C. (39.2 deg. Fahr.) The particles now, instead of becoming denser, actually expand, and so remain at the top, until a thin layer of ice is formed. This is exactly what takes place in our lakes and ponds every frost, the circulation continues until the whole mass attains the temperature of 4 deg. C. (39.2 deg. Fahr.) when it is gradually and finally arrested; a thin layer of ice is then formed at the top, acting as a cloak to the interior, which remaining always at 4 deg. C. (39.2 deg. Fahr.) preserves the animals and fishes from the action of intense cold. Were it not for this fact, our lakes and rivers would all be frozen at the bottom, and as water is such a bad conductor of heat, they would in time be converted into one solid block of ice, which would defy the hottest rays of a tropical sun to melt. Thus we see that such a wise provision of Nature depends entirely on an apparent exception to a universal law, which is so slight that it requires the most delicate experiments to detect it.

This fact was first noticed by the Florentine academicians in 1670, and was carefully investigated at the end of the last century by Gineau, while determining the French unit of weight. His results were confirmed by Hallstrom and Despretz in 1839; but the most recent experiments are those by Plucker and Geissler, and though their results varied between 3.8 deg and 4.5 deg C. (38.8 deg. and 40.1 deg. Fahr.) we may take for all practical purposes, and without any sensible error, 4 deg. C. (39.2

deg. Fahr.) as the point of maximum density.

Having now arrived at the freezing point of water, we will briefly discuss the different phenomena in connexion with it, and sum up with a few of its consequences. The freezing point of a liquid is almost invariably the same as its melting point; that is, if we cool a liquid below its melting point, it will become solid. There are of course many exceptions to this, and even water has been known to be cooled down to 20 deg. C. (4 deg. Fahr.) without freezing. To effect this, however, the water must be kept perfectly still, for with the least vibration congelation commences, and the temperature will instantly rise to zero. An interesting experiment to illustrate this fact, is to take a flask filled with a warm strong solution of sulphate of soda, and allow it to cool in a perfectly still place. The solution will cool down to several degrees below its freezing point, and still remain liquid; if we now sound a deep note on a violin, close to the flask, the whole solution will form almost instantaneously into beautiful crystals, the temperature at the same time rising. If, however, the vibration be not sufficient to cause this change, by dropping the smallest crystal into the flash, the result is almost certain. If we try this experiment we are nearly sure to succeed, but in trying to cool water below its freezing point we very often fail for this reason. The fact that we can do it at all with any liquid, is mainly owing to the inertia of the particles; and as the solution of soda is less fluid than that of water, it is consequently much more inert. By mixing salt with water we lower its freezing point, sea water freezing at—3 deg. C. (26.6 deg Fahr.) and a saturated solution of common salt and water will not freeze until it reaches—20 deg. C. (4 deg. Fahr.) A curious illustration of the fact, that by mixing different substances, we reduce the melting or freezing point below the mean of the two substances, is seen in the common fusible plug, which consists of the following alloy:

2 parts	bismuth,	whose melting point is		495° F.
1 "	lead,	"	"	612° F.
1 "	tin,	"	"	440° F.

The resultant melting point of this alloy is only 96 deg. (204 deg. Fahr.), being far below that of each of its component metals.

When a substance solidifies or freezes, there is always a change of volume, which usually is a contraction; but in the case of water and a few metals, such as cast iron, antimony, bismuth, &c., an expansion takes place. The expansion of water at the freezing point is by no means gradual, but it takes place almost instantaneously, and the amount of force exerted at the time is enormous. After the water has gradually expanded from 4 deg. C. to zero (39.2 deg. to 32 deg. Fahr.), at that point it suddenly increases to about one-tenth more than its volume at 0 deg. C. (32 deg. Fahr.)

Of the enormous force exerted by freezing water, we have ample testimony in the bursting of our water pipes, boilers, and other sadder calamities; but it is a very simple matter to calculate pretty accurately the amount of force developed. We have previously stated that Grassi proved the compression of water to be proportional to the pressure; he also found from very accurate experiments, that with a force of one atmosphere, or 15 lb., water was compressed 0.0000503 of its original volume; and knowing that water exerts in expanding a certain volume, a force equal to the one required to condense it to that amount, the following proportional sum will tell us pretty accurately the amount of force exerted by water when freezing:

As 0.0000503 : 0.1 : : 15 lb. : 29,821 lb.

That is to say in freezing, water exerts a pressure of about 30,000 lb. per square inch, which far surpasses the strain that any of our machinery or structures are calculated to bear, and no wonder then that under its mighty pressure they give way.

We will now say a word or two about the question which really gave rise to this paper, and that is, what are the effects produced upon a boiler, supposing it to be filled with water and left to freeze? We will not make any hypotheses, but think we cannot do better than lay before our readers the result of an experiment that has been tried, and which is so simple that any one can try it for himself, and thus form his own conclusions. A small tin model of a

boiler was made of the following dimensions: length 9 in., diameter 6 in., flue 3 in. diameter, this was filled with water by means of two small taps, one being used to let the air out. When filled the model was carefully measured all over, the diameter of the shell and flue being taken accurately and then placed in a freezing mixture of common salt and pounded ice. In a short time, a slight disturbance was noticed in the box containing the mixture, and it was found that the model was a wreck, the shell having given way at the soldering, the end being partially liberated. The pieces were carefully taken away from the block of ice, and upon coming to the flue, it came out of the block quite easily, and was not bound in the least. Upon measuring the block, the outside diameter was of course larger, but so was the diameter of the flue, showing that there had been no pressure on the metal, as the ice did not anywhere touch it. Upon consideration, we shall find that these results are perfectly in accordance with all the laws we have previously enunciated. As the flue is exposed to the action of the freezing mixture equally with the shell, it follows that a layer of ice is formed around the flue simultaneously with one next the shell. That is to say, the circle of particles around the flue will be increased in diameter, and if the particles in the first instance touched the flue, they cannot possibly do so in their new form. Thus we see that while the circle of particles in increasing its diameter at the shell, tends to burst the boiler; in expanding at the flue, it of course tends to leave it, and the plates and joints of the flue are thus relieved of all pressure. If the pressure were exerted when the water was in a fluid state, the pressure would no doubt fall equally upon the flue, but as the sudden expansion takes place in the act of solidifying, the thickness of the ice increases with the pressure, and thus the flue is gradually protected.

SUPERHEATED STEAM.

From "The Engineer."

If low-pressure steam is to compete in economy with high-pressure steam, it must be used under special conditions. The experience of more than a fourth part of a century has proved unmistakeably, that with saturated steam of 25 lb. absolute pressure, or thereabouts, cut off at half-stroke, in large unjacketed cylinders, we cannot get a horse-power with a much smaller consumption than 5 lb. or 6 lb. of coal. In the face of this fact, it requires, we admit, some temerity to argue that it is possible to work an engine with steam of no greater pressure on a consumption of less than half 5 lb. of coal per horse per hour. Yet it may be shown that, so far as theory is concerned, there is no absurdity about the argument; and it may also be shown, we think, that no insuperable difficulties stand in the way of carrying theory into practice in this case. It is well understood now that there is no source of loss of efficiency which can compare in importance with cylinder condensation. If only steam could be maintained in a cylinder as steam, and not as aqueous vapor or mist, or suspended or deposited water, then the highest degree of economy which theory points out as possible of attainment with any ratio of expansion might in practice be attained. To guard against condensation, cylinders are carefully clothed and jacketed; but even in the most economical engines which it is possible to construct, it is indisputable that a large percentage of the steam admitted to the cylinder is condensed, the condensation having nothing to do with that due to the performance of work. In dealing with this fact we are prepared to admit, that it is impossible for any one to say with precision how much steam is wasted in this way; we have, however, data before us derived from elaborate experiments made with an engine having a 50 in. cylinder and a stroke of 8 ft. 9 in., in which the condensation amounted, when the steam was cut off at one-fourth of the stroke,

to 46.5 per cent.; when the steam was cut off at one-third the loss fell to 28.7 per cent.; when cut off at half-stroke the loss was 25 per cent.; and when the cut-off took place at 58 per cent. of the stroke the loss was 18.74 per cent. The engine used at the last-named point of cut-off 32 lb. of steam per indicated horse-power per hour; so that the loss by cylinder condensation amounted to over 6 lb. per horse-power per hour under the most favorable conditions. The cylinder was well clothed but unjacketed, and the total initial pressure of the steam was 40.6 lb. We could cite other examples taken from various experiments, if it were necessary, all tending to prove the proposition which we are about to put forward—namely, that under most favorable conditions the loss of steam due to cylinder condensation amounts to somewhere between 4 lb. and 6 lb. per horse per hour. We shall not, we think, under the circumstances, err much if we assume that 5 lb. of steam per horse per hour are lost through cylinder condensation alone in high class engines working with saturated steam, and this without regard to the condensation due to the performance of work, but including all steam condensed in jackets. Now it is a very good engine indeed that can manage to get an indicated horse power per hour out of 20 lb. of steam. Of this, 5 lb. are wasted by cylinder condensation in nine cases out of ten. It follows that we burn 20 lb. of coal instead of 15 lb., and that the rate of consumption per horse power per hour is 33 per cent. higher than it might be if cylinder condensation were totally prevented. If we take the 33 per cent. as a factor, and argue that if the best compound engine working with 75 lb. steam wastes 33 per cent. of fuel, we have a margin available to cover losses from any other source in an engine working under different conditions, but in which cylinder condensation does not take place. In other words, if we have two engines, one working steam expansively under given conditions and the other not—the latter, however, not suffering from cylinder condensation—then, unless the gain due to expansion represents a greater gain than 33 per cent. on the working of the non-expansive engine, the latter may prove the more economical of the two. To put this numerically, if the expansive engine uses 20 lb. steam per horse-power per hour, of which 5 lb. are wasted by condensation, then a non-expansive engine using any quantity less than 20 lb. of which none is wasted, will compare favorably with it. We may, so to speak, set the advantage won by averting cylinder condensation against the gain which results from working high-pressure steam very expansively. To make this point perfectly clear, let us suppose that we have a cylinder fitted with a piston having an area of precisely one square foot. Then, if we admit one-eighth part of a cubic foot of steam of 75 lb. total pressure below the piston we can lift a weight of 4147 lb., omitting fractions, through a height of one foot. As used under these conditions, a cubic foot of steam will lift 33,176 lb. a foot high; but we shall assume here for convenience that 60 cubic feet of 75 lb. steam expanded eightfold will develop in round numbers on horse-power. The 60 cubic feet of steam will weigh 10.74 lb. It is well known that it is impossible to get a horse-power in practice out of 10.74 lb. of steam. As we have said, the quantity required in practice cannot be much less than 20 lb. In a very accurate series of experiments carried out with a pair of compound pumping engines at Chatham Dockyard in 1874, the consumption of steam reached 18.92 lb. In one set of experiments with the American steamer Rush with compound engines it amounted to 18.38 lb.; in another experiment to 22.09 lb. If it were necessary we could go on adding example after example to prove that to get a horse-power out of 20 lb. of steam is an exceptionally excellent duty. We shall not attempt to prove here once more that an eightfold expansion of 75 lb. steam gives about the best possible results. We have the difference between 10.74 lb. and 20 lb. to account for, and the sources of loss may be classed under two heads—loss due to wasteful cylinder condensation 5 lb.; loss due to clearance, leakage, steam required to overcome back pressure, &c., 4.26 lb. We are perfectly aware that the loss by cylinder condensation *may*, under certain conditions, with very carefully jacketed cylinders, be as small, perhaps, as 2 lb.

per horse per hour. In such instances, however, the engines are probably working with less than 20 lb. of steam per horse per hour. Thus in the case of the Rush, when using 18.38 lb. of steam per horse-power, 93 per cent. of the water was accounted for by the indicator. The loss by condensation was thus but 7 per cent., or little over 1¼ lb. per horse-power per hour; but when the engines were using 22 lb. the loss reached nearly 25 per cent., or over 5 lb. per horse per hour. It would be but waste of time to adduce further arguments to prove that when an engine is using 20 lb. of steam per horse per hour, one-fourth of the steam is simply wasted in keeping the cylinder hot.

Let us now compare the case we have stated with the conditions which obtain when an engine is working at a disadvantage as regards the pressure and measure of expansion used, but at an advantage in that no condensation takes place in the cylinder. Steam with a pressure of 25 lb. is admitted to a cylinder having an area of a fraction over 1.65 of a foot, and is cut off at one-third stroke. The average pressure will be 17.5 lb., and the work done in lifting the piston 1 ft. will be, as in the first case, 4147 foot-pounds, but the volume of steam used instead of being one-eighth of a cubic foot, will be 55 of a cubic foot, and to produce a horse-power we shall have to pass 264 cubic feet of steam, weighing 16.73 lb., per hour through the cylinder. Now on the hypothesis that no condensation takes place, in this case, in the cylinder, and that the remaining losses will be the same in both engines, we have to add to 16.73 lb. only 4.26 lb., making the total 20.99 lb.—say 21 lb. From this it appears that if cylinder condensation could be wholly averted, a low-pressure engine working with a very moderate grade of expansion would be within 5 per cent. as economical as a high-pressure engine of the best type now in use.

Of course we shall be met at this point by the objection that if cylinder condensation can be avoided in one case it can be avoided in the other, and that the high-pressure engine will still, under the new conditions, be the better of the two; but a little examination of facts will overset this theory. In practice it is found to be impossible to avert cylinder condensation by any known expedient but one. Instead of using dry saturated steam we must use steam gas, or, in other words, superheated steam. But superheated high-pressure steam cannot be used in practice for reasons which are well understood by engineers; and the question has yet to be solved whether it is or is not possible to use it with low-pressure engines. All our arguments are based on the assumption that it is possible, and we shall endeavor in another article to demonstrate this prominently. In the mean time, however, it is expedient to notice the late Professor Rankine's investigation of the properties of steam gas. It will be seen that we claim for it the power of reducing the consumption of steam by about 25 per cent., or from 20 lb. per hour to about 15 lb. Now, Rankine's investigations go to show that when superheated steam is expanded without gain or loss of heat—a condition which may be said to be approximately attained when it is used in an unjacketed but well protected cylinder—the gain under favorable conditions is but 15 per cent. On the other hand, however, he has also shown that when superheated steam is used at a constant temperature, the saving may amount to as much as 27 per cent. It would be impossible at the end of an article like this to deal with the questions which have to be considered in discussing the possibility of using low-pressure steam gas. All that we have endeavored to do, so far, is to prove that if cylinder condensation be averted, low-pressure steam will give as economical results as high-pressure steam, It is not necessary to prove that cylinder condensation cannot be averted by any other expedient but superheating. All experience goes to show that high-pressure highly-superheated steam cannot be used. It is very confidently asserted that the low-pressure highly-superheated steam cannot be used satisfactorily. This last is the question which we wish to see re-opened. We have endeavored to prove that great advantages would be gained in the matter of economy if it can be used. It remains for us to adduce such arguments as we may to prove that superheated low-pressure steam may be used successfully and economically.

THE RUSSIAN CIRCULAR IRONCLADS.

From "Engineering."

THE interesting letter from Mr Reed which appeared in the *Times* ought to attract public attention forcibly to the circular ironclads of the Russians, as well as to some other points in their naval system which Englishmen may study with advantage.

Mr. Reed has made it, in fact, the naval question of the day whether we ought not to follow in their footsteps, and build ourselves some of those extraordinary vessels to which the name "Popoffka" has been applied in Russia. The invention of circular ironclads, as is well known, is due to the late Mr. John Elder, but it was left to a Russian, Admiral Popoff, to see its value, and to put it, with some modifications, into practice. Both inventor and adapter appear to be justified by the result, for it seems hardly possible to deny that for a given sum of money an ironclad of much greater defensive power, and of greater offensive power too, so far as guns are concerned, can be produced on the circular than on any other principle.

The Admiral Popoff, the second circular ironclad of the Russians, is 120 ft. in diameter. She draws about 13 ft., and displaces some 3500 tons—a very small displacement for a vessel whose armor is equal to 18-in. plates, and which carries two 41-ton guns. The freeboard is 18 in. only, but the guns are carried in a central tower (which might be a revolving turret if preferred), and deck superstructures may be added to almost any extent. Altogether there seems no reason to doubt her seaworthiness, or even dryness. Her stability, of course, must be beyond dispute, and for steadiness as a gun platform she ought to be unapproachable—the ordinary connection between stability with unsteadiness not existing in the case of vessels of very great breadth and light draught, as has been shown in the American monitors, and more lately in the Dicey steamer. That the Popoffka will be handy to an unprecedented degree is obvious. There is no reason why they should not be constructed as rams, and their extreme handiness and rounded form, as well as the facility with which they can be subdivided internally, should make them safer than most vessels against the attacks of other rams. It is also remarked by Mr. Reed that they lend themselves with peculiar readiness to complete armoring of the sides down to the level of the bottom, should that extension of the present "water-line belt" ever be deemed desirable as a protection against torpedoes. With so many independent propellers and sets of engines the ship can hardly ever be left without motive power, except through absolute failure of coal. As already stated, the draught is light, and, the displacement, or in other words the cost, is very small relatively to the fighting power obtained.

In every point mentioned, and we believe we have named all of any importance save two, the circular ironclad is either equal or distinctly superior to any ironclad of ordinary form and of the same cost. The two points reserved are efficiency under sail, and speed under steam. As to the first, no heavy ironclad can make the slightest pretence to efficiency as a sailing ship, and the most that can be done for any of the class is to furnish them with some kind of jury rig that might (with a fair wind) help them into port after the coals had given out. Unpromising subjects as the Popoffka are for a sailor's enthusiasm, it would probably be possible to do as much for them in this direction as for the Devastation or Inflexible.

The question on which their place in naval warfare really depends is that of speed under steam. The proportion of steam power to displacement in the Admiral Popoff appears to exceed that in the fastest of our modern ironclads, but although the anticipated result is not known to us, we presume the speed will not exceed eight knots, even if it reach that figure. What engine power would be wanted to drive such a vessel at 13 or 14 knots is a question beyond profitable calculation at present, until more is known of the existing Popoff-

ka's performances, but the engines which could give such a result, and the coal which could keep it up for a reasonably long period, could only be carried in a very large and costly ship. Still, the Admiral Popoff displaces less than one-third as much as the Inflexible, and more than 200 per cent. is a wide margin to work with. If any man is bold enough to conceive a Popoffka traveling at 13 knots an hour, let him see what he can produce on a displacement of 11,000 tons. If he can only gain the speed he is sure to beat the Inflexible in everything else. In that case we shall have found our model for "line-of-battle ironclads," and the Popoffka's place in warfare will be a very high one. The coal bill will be heavy, and docks and dock gates will want a good deal of reconstructing, but the model will be found beyond possibility of cavil.

But the circular ironclad may still prove to be the right model for our heavy fighting fleet, even though no such speed as 13 knots be attained or attempted. It is not clear that this high speed—which is about that of the Devastation—is necessary, as we have often pointed out ;and though the question is fairly open to argument, it is possible that to secure such advantages as the Popoffka offer us, we might be content with less—say with 10 knots. If it be possible to build for the cost of the Devastation two Popoffka of only the some armor and guns as the Admiral Popoff carries, and of say 10 knots speed, it is hardly open to doubt which would be the most profitable way of spending the money. A hostile Devastation could elude them of course, but she could not fight them (or either of them, probably) and thus they would have at least the "command of the seas." The French fleet of old were nearly always better sailers than our ships, and for years were able to keep out of our way as they pleased. Yet so little did this answer their purpose that they offered a meeting at last, and were destroyed, and the same thing would almost certainly happen again. If not, it should be remembered that though the best thing to do with your enemy is to take him or sink him, the second best thing, and no bad substitute, is to make him run away. Speed may not be indispensable, however desirable, in a line-of-battle ship.

These considerations as to the employment of circular ironclads as substitutes for the present type of seagoing ships are offered merely to check the too common assumption that they have no claim to attention, except for the purposes of coast defence and attack. We have no wish to drive the argument too hard, or to pretend that such considerations as we have offered are otherwise than speculative, and we do not forget that powerful authorities are opposed to the building of heavy ironclads of any kind whatever—though of those who take this, as we are convinced, grievously and dangerous mistaken view, some might perhaps be willing to accept a sea-going fleet of Popoffka (assuming a sufficient speed) in lieu of the less well-defended ironclads they now object to.

Even more extreme opponents of armor than these appear not unwilling to discuss the merits of the Russian ships for coast defence, so we will next consider them in that character, *i.e.*, as vessels perfectly able to make passages from port to port, or to cross the seas in safety when required, but disqualified by their low speed from the usual operations of sea-going ships.

Our most recent ships of the coast-defence class are the Cyclops and three sister vessels, all of which were commenced on the outbreak of the Franco-German war. The Cyclops displaces 3430 tons, *or about the same as the Admiral Popoff*, has 8-in. and 9-in. armor (with some thinner), and carries four 18-ton guns. Her speed is ten knots and her "coal-endurance" small, though probably this is quite as great as the Popoff's. Her seaworthiness has scarcely been tested, but doubt has been thrown on it. She can certainly move from port to port more quickly than the Russian ship ; but in actual service, say in defending a particular harbor or estuary, or bombarding a fortress, her higher speed would rarely, if ever, give her any advantage. *Au rest*, she has 8-in. armor against 18-in., and (four) 18-ton guns against (two) 41-ton guns, and she draws 15 ft. 6 in. against 13 ft. and is much less handy. The Admiral Popoff would have as much to fear from a

Chinese junk as from the Cyclops, and it is pretty certain that no modification of the latter, no concentration of the guns in one turret, no introduction of coal-tank ends, no shortening and broadening, short of absolute or nearly absolute roundness, could ever put her on a level as to fighting powers with her opponent.

Thus the circular ships appear to be perfect for coast-defence purposes, if it be accepted—as we hold it should be—that speed is comparatively unimportant for such duties. The question of course remains whether we require coast-defence ships at all, or whether our coasts can be better defended by fixed fortifications or by the sea-going fleet. It would be useless at the close of the present article to enter upon the discussion of such a wide question as this—a question, moreover, of naval strategy, not construction. It will be sufficient to say that although, since our coasts are widely different from those of Russia (which are indented with numerous shallow inlets), what may be exceedingly wise policy for her might be foolish for us, our own opinion is that we require a certain moderate number of such vessels for service in the estuaries of the Thames, Mersey, Humber, Tyne, Clyde, &c. The great naval stations like Portsmouth and Plymouth are, or ought to be, sufficiently defended by the fleet and the fortifications, while minor commercial ports can have a cheaper defence in the form of earthworks or Moncrieff gun pits. A dozen circular ships like the Admiral Popoff would be a magnificent addition to the power of the country, and ought effectually to protect all our great commercial ports from injury or even insult. It is not probable that such thoughtful and ingenious men, as the Russian constructors have proved themselves to be, have left any stone unturned to satisfy themselves that the form they have adopted is the best. Substantially (subject to the questions already raised), we have no doubt that it is so, but we should like to know whether the experiment has been tried—or is to be tried—of attaching a wedge or cutwater at each end, sufficient to give water lines making an angle of about 45 deg. with the fore and aft centre line; this would add little to the length of the ship. The wedge-shaped additions might be external to the armor, the circular plan of which would be preserved, and might be used as coal bunkers or coal tank ends. We should not be surprised to find that so very bluff a bow offered practically nearly the same resistance as the round one, but the experiment would be interesting.

THE DELTA OF THE MISSISSIPPI VALLEY, CONSIDERED IN RELATION TO AN "OPEN MOUTH RIVER."*

By JOHN G. BARNARD, U. S. Corps Eng.

Transactions of the American Society of Civil Engineers.

To furnish an outlet to the Mississippi which should evade the bar obstructions of the mouths, a "ship-canal" had been proposed, first in 1832, by Mr. Benj. Buisson, State Surveyor; and again, in 1838, by Maj. W. H. Chase, U. S. Engineers; and the project was referred to by the Board of 1852, as one to "fall back on" in case of failure of all efforts to procure an open mouth; thus placing it in its rightful category of a *dernier resort*. This project was revived by the following resolution of Congress, passed March 14th, 1871:

"Resolved: That the Secretary of War be, and is hereby requested to cause an examination and survey, with plans and estimates of cost, to be made by an officer of Engineers, for a ship-canal to connect the Mississippi River with the Gulf of Mexico, or the navigable waters thereof, of suitable locations and dimensions for military, naval and commercial purposes, and that he report upon the feasibility of the same to the House of Representatives."

* Abstract of a paper presented to the American Society of Civil Engineers.

In the summer of 1873, a Board of Engineers of which the writer was President, was convened "to consider and report upon" a plan which had been in the meantime prepared in fulfillment of the above. The question submitted to the Board was widened by the request of the Chief of Engineers, at the suggestion of the President, to consider "the expediency of improving the navigable outlet of the Mississippi, by the Fort Saint Philip Canal, as an alternative to, or a simultaneous measure, perhaps, with, the improvement of the passes."

The majority of that Board favored the construction of the canal and embodied their views as to jetties in the following paragraph:

"Upon a review of the practical difficulties which the adoption of the jetty system of improvement at the mouth of the Mississippi would entail, and a due consideration of the original cost of construction and of annual extension, entertaining doubts, moreover, of the successful issue of the attempt, the Board do not consider it advisable to recommend it."

The single dissenting member took the ground "that before resorting to an artificial work of the difficult and costly character of a ship-canal, a more attentive consideration of the superior advantages of the natural mouths, and of the fair probabilities of utilizing them, is needed."

And again: "the advantages of an open river mouth are inestimable. The needs of a navigation so great as that which now exists, and which in the future of the great Mississippi Valley must be fifty-fold increased, demand it."

"It is said that the 'time has come' when the needs of commerce demand the canal; but I answer that the *time will come* when there will be the same cry for navigation unimpeded by locks—an *open river mouth*—which we now hear for a canal."

The argument, as put in this last quotation, contains the very pith of the question. No seaport in the world would substitute a ship-canal, with locks, for an open sea entrance, if the latter be not *unattainable*.

Would New York, for example, accept as adequate a ship-canal, for her sole channel of access to the sea? And what limit shall be placed upon the magnitude of the freight-commerce which shall pour out of the great Mississippi Valley through its only outlet to the sea?

A ship-canal can be but a make-shift, a *pis aller*, only to be accepted on *proof* that there is no *reasonable hope* of an "open mouth." How shall that reasonable hope be established save by reference to what *has been done* elsewhere, and by an appeal, not to theories—for theories simply mock the subject by their impotence to grasp the complicated conditions—but to such simple elementary *facts* as may guide our estimate of probabilities? Here all the burden of proof is thrown on the opposer.

To say, in face of the highest authorities* on this subject, in face of *achieved success* in numerous instances, that there *are no probabilities*, or that there are none which justify an attempt which has for its end a result so indispensable to a great navigation, require not merely transcendent engineering abilities—it requires something like *prescience*.

The question is *not*, therefore, purely an engineering one. To fail (if the contingency of failure be admitted) is far from being an *engineering* failure; for the undertaking is not merely justified, but *demanded;* unless, indeed, the engineer can deny even the *possibility* of success.

While no engineer, it is presumed, would trust his prescience so far as to deny *possibility* of obtaining by jetties an "open river mouth," there are a few simple arguments on which to assert—*first*, the strong probability of success; *second*, that the *maintenance* of the accomplished open mouth need not be regarded as involving an unreasonable expenditure or an excessive amount of work.

As to the *first point;* no one, whether "engineer" or otherwise, has yet denied the certainty of *obtaining* by jetties the desired result.

* In Europe, *e.g.*, we have Sir Charles Hartley, engineer of the successful jetty construction of the Sulina; Col. James Stokes, Royal Engineers, British Commissioner for same work; and Mr. P. Caland, engineer of the works at the mouth of the Maas; all advocating an "open mouth" for the Mississippi; nor can the opinions of these eminent engineers be impugned by the allegation that, being foreigners, they are unacquainted with the physical peculiarities of the Mississippi. They have all studied it attentively with access to the most authoritative information; including the work, the "Physics and Hydraulics of the Mississippi."

Hence the *second point* only, requires to be dwelt upon. In the draft of a minority report, dated New Orleans, December 6th, 1873, this matter is thus set forth:

"Now as to the application of this work. For reasons which will appear hereafter, I should select Pass á Loutre for experiment. A glance at the chart will show that from the point in the Pass where the depth of 25 feet ceases to obtain, to the *outer crest of the bar*, is about 2½ miles (at the South West Pass over 4 miles). The natural width of the Pass where 25 feet depth obtains is about ½ mile). * * * By stopping the North Pass, and extending parallel, or nearly so,* jetties which, starting at the cessation of 25 feet depth (above the North Pass), extend, a half mile apart, 4 miles to points opposite the outer crest of the bar, the bar must be excavated to 25 feet (*i. e.*, the 25 feet channel will be extended at once 2½ miles); the velocity of current maintained unimpaired up to this point will carry its sediment far beyond, into deep water. The present regime of a shoal bar cannot again be restored, until the vast bottom area now covered with deep water beyond, for a distance of 2½ miles, is raised. That there will be a 25-feet, or even a 20-feet available channel *all* this long time I do not contend. But this I do contend, viz.: * * * that once extended out to the crest of the existing bar, jetties do not require the incessant *following up* supposed; that they may *ultimately* require extension I do not dispute."

The recent Board of Engineers (of 1874), to which the whole subject was recommitted, used, in giving its decision in favor of jetties, substantially, but quite independently, the same argument, though more fully and clearly developed.

"At present, the muddy water issuing from the South Pass spreads out in somewhat of a fan-shape, the handle of the fan being at the mouth of the pass and the ribs several miles in length."

"If the proposed jetties were instantly completed, and the new channel scoured out, essentially the same amount of sediment would be spread out in fan-shape, but, from the greater velocity of the issuing water, the ribs of the fan would be longer, while the handle would be narrower. More of the sediment would at first be deposited far out in the Gulf than before.

"But with the present rate of advance, the 25 feet curve 120 years ago was about 12,000 feet above its present position; and if the volume of water carried by the pass is kept the same, neglecting the slight difference in slope of the Gulf bottom outside the present bar, in about 120 years a new end for the pass will probably be formed of the same general shape as the lower 12,000 feet of the present pass. It makes little difference, in the whole time required to accomplish the work, whether the same volume of water flows out at starting over the present shallow bar or from beneath two dikes which force the water to take a depth of 30 feet. In an average of many years, the rate of progress must be about the same as now, namely 100 feet per annum, the volume of water being kept as at present; and it is on this basis that the average annual cost of extension, namely, $130,000, has been computed.

"It has already been stated that it is proposed to obtain a depth of 30 feet. between the jetties, in order that some years may elapse before the shoal which will form beyond the jetties can have on it less than the required depth of 25 feet in the channel through it. There are no precise data for estimating this period. Going seaward from the upper end of the proposed dikes, the slope of the bottom of the South Pass is about $\frac{1}{440}$. This slope doubtless depends mainly on the velocity of the water flowing through it and on the lifting of the fresh water by the salt. As the causes remain essentially the same, it would seem natural that the new end of the South Pass to be formed by the sediment passing through the jetties should at least have the same bottom slope. If this assumption were true, the bottom would at least shoal from 30 to 25 feet in a distance of $5 \times 440 = 2{,}200$ feet, and the time required would be about twenty-two years."

Mr. Eads in his pamphlet entitled "The Jetty System Explained" (printed in 1874), pages 6–7, dwells on and develops the same principle, justly reasoning from the facts of nature, that be-

* "Perfect parallelism is not necessary; large deviations may be made to select the best location."

tween the lowest point at which there is 60 feet depth in the South West Pass (whether that *final* 60 feet be in the unmodified pass, or suddenly realized, by means of jetties, on the site of the *present* bar) and a bar having only 15 feet, there must be a distance of $7\frac{1}{8}$ miles—and the *time* for the creation of a new bar (if by jetties the present bar be deepened to 60 feet) must be equal to the number of feet in $7\frac{1}{8}$ miles divided by the amount in feet of the *present annual* advance; a quotient which he assumes to be 178 years.

The arguments are *essentially* identical in all the three independently exhibited forms. These reasons are in themselves the broad and indisputable *facts* of observed configuration. All that has, in a different and conflicting sense, been arrayed is—where not pure assumption—based on theories* which have themselves, no sufficient inductive basis.

But the matter may be somewhat otherwise stated, thus—the passes can prolong themselves no faster than they can build up their base of formation; the broad *bank* on which their bed is laid and that serves as the solid *trough* (as it may be called) which conveys them to the sea. The *front* on which the fan-like expansion over which the great delta arms are laid, has an extent measured on the 100 feet (depth) curve of about 40 miles. The material if all laid *within* (and we know it is transported far beyond) the 100 feet line, cannot advance this front much more than 300 feet per annum. Or if we take the South West Pass by itself (forming a kind of salient on the western extreme of the general configuration) its based measured at the same depth is 10 miles, (even on the 18 feet curve the base is 5 miles,) and this same configuration obtains whether we refer to Talcott's chart of 1838 or the Coast Survey chart of 1867, during which interval there has been a *general* progression of $1\frac{1}{2}$ miles. The side banks, from the pass to lateral deep water, are quite pertinently compared by Mr. Eads to completed natural jetties; and as such, they must complete themselves as they advance. There is every reason to affirm that the advance, whether the material be projected with high velocity over a bar crest 2,000 feet long, or with low velocity over one 11,500 feet long, cannot be made on a base of *less width than we observe to be essential to the natural advance.**

Stress is laid on this exhibition of the case because it discards all theorization, and all subsidiary and disputable questions of "littoral currents," "tide," "winds" or "waves." If in the final minority report of the writer stress is mainly laid upon *precedent*, it is because successful precedent *is enough* to prove the claim that the "open mouth" should be striven for at the Mississippi; and because the greatest possible brevity was aimed at.

It will be noticed, doubtless, that in the passage quoted from the minority report as first drafted, preference is given to Pass á Loutre, not because the analogy between the South Pass of the Mississippi and the "Sulina" (both bearing about 8 per cent. only, of the total discharge) had not already attracted the attention of the writer; but because in sketching out, under great pressure as to time, some outline of an application of the jetty system to the Mississippi delta, it was deemed inexpedient to select an obscure pass—one which has never been known to navigation—for the purpose. Pass á Loutre is, on the other hand, in some degree a rival of the South West Pass, while the application there would involve very much less expense than to the latter. Preference was subsequently given to the South Pass for reasons fully set forth in the minority report.†

In the foregoing, I have endeavored to give a concise history not only of actual operations at the delta mouths, but of the various projects for providing an adequate outlet, whether by operating on the mouths themselves, or by canaliz-

* The two most prominent and sharply defined "theories" of bar formation are those of Mr. Ellett (the vertical *eddy* theory), and of the "Physics and Hydraulics of the Mississippi," pages 445-6, 7, 8, refuting the former and attributing the bar formation to the heavier sediments "pushed along the bottom," but which also calls for "vertical eddies" and a "dead angle, where the river water meets and rises upon the salt water." Were this latter theory *proven*, it would be far from sustaining the inverse rule-of-three computations of enormous bar *advance*, under influence of contraction by jetties, based on it in Ex. Doc. 220, page 31.

* The following from the "Physics and Hydraulics of the Mississippi," page 449, is but another *form* of the above affirmation: "The oscillating motion of waves, when meeting bottom, is changed into a motion of translation, and this tends to arrange the deposit made by the river *into the same gentle slope at which it disposes similar material at corresponding depths along the shores.*"

† Ex. Doc. 220, page 124.

ation. I have, at the same time, endeavored to make plain the grounds on which an "open river mouth" may be confidently hoped for.

In this matter the recent Board (of 1874), in all essential matters, is in unison with me. Its plans or its estimates are doubtless able and thorough, and in absence of any precedent for the kind of work it proposes, are naturally founded on the best foreign precedents, which are those furnished by the practice of Holland, and more especially by recent works at the mouth of the Maas.* These works were visited by me in 1871, and fully described with full details of the facine and ballast construction ("zinc stukken, &c.) in Professional Papers, Corps of Engineers, No. 22, and the methods indicated as likely to "prove especially available in Louisiana." But at the same time they can but serve as *models*. In practice and adaptation to the peculiar locality, I believe they may be much simplified and the desired results of the jetty application be arrived at more speedily and economically than by following rigidly the Board's model.

In all that relates to the rival project of a canal, the recent Board has given incomparably the best solution of that problem yet offered. Nevertheless, while always admitting the *practicability* so far as the mere question of construction (a difficult one though it be), I am far from believing it to offer a reasonable assurance of success, if by *success* it meant the furnishing of an outlet at all adequate; I doubt much, whether in its *working*, it will fulfill even that degree of success which would be justified for similar canals in other localities, and I feel sure it could not be maintained for the small annual sum estimated.

Before closing this paper, I would advert to the case of the Rhone mouth, upon which much stress has been laid as an unsuccessful attempt to apply jetties. I have before me a copy of the "Rapport sur les Projets presentés par les Ingénieurs chargés de la Navigation du Rhone,"* made by a "Commission" of the "Ponts et Chaussées."

Stating that the General Council (*Conseil General*) of the Ponts et Chaussées has already been engaged with this subject in connection with two projects presented by Mr. Surrell, it mentions the *first* as that for a "canal maritime," &c., &c., and the *second*, as one for *concentrating the waters of the Rhone in one single arm*, that of the "Grau de l'Est;" the Council referred to, stating the principle upon which rested this project to be, "that the depth over the bar of a river mouth emptying into the sea is the greater, the greater the quantity of water which is discharged through it."

Thus we find laid down authoritatively, at the very outset of this matter, not the *jetty* method but (see enumeration, page 107, made by the Board of 1852) a distinct and very different one, *i. e.*, that, specified in the words of the board just cited, as "closing the useless passes." And, indeed, through the forty closely written manuscript pages of the report before me, this, and this only, is dwelt upon and discussed in all its bearing, without even an allusion to the very distinct notion of jetties or, as more generally known in Europe, that of "parallel piers."

That the actual construction assumed somewhat the semblance (as far as they went) of "parallel piers," is due simply to the fact that it was deemed indispensable to connect the *dams*, which stopped the "useless passes," by dikes running over the low, intervening islands (*theys*) —that, on the east side it was found easier and more economical to make a *continuous* dike, riverwards of the islands, than isolated dams.

Finally, this Commission, concludes as follows: "Whereas," * * *

"3d. The delta arms (*graus*) of Pié manson and Rouston, which are the first derivations, up stream, from the main river, receive more than half the total discharge.

"4th. That the closing of these arms will constitute a valuable experience from which may be derived useful information.

"Therefore, the works to be executed

* These, even in Holland, were the *first* applications of this kind of work to an *open sea exposure*. So successful has it proved that the lower legislative body of Holland reports that "*the complete success of the works at the Hook of Holland has removed all doubts as to the possibility of making piers at sea*" (*i. e.*, jetties) "*on our coast.*"
The construction, wholly of beton blocks at the sea entrance of the new North Sea Canal, has not proved so complete a success.

* Furnished to Gen. Wright, President of the Board of 1874, by M. Malezieux.

for the improvement of the Rhone mouth should be confined to the closing of the Piemanson and Rouston arms."

The government, however, decided to carry out the plan of Mr. Surrell in full; and now let us hear in the language of M. Malézieux (September 10th, 1874) *what* was done.

The works constructed constitute a continuous dike ("*endiguement*") upon the two banks of the Rhone, from the Tower of St. Louis to the bar; that on the left has a total length ("*developpement*") of 7 kilometres (4½ miles), and *stops at* 1,531 *metres* (a mile less 80 yards), *inside the crest of the bar*; that on the right has a length of 6,500 metres (4 miles), and stops at 1,460 metres (⅞ mile) *inside the crest.* * * * "The result of these works was to *concentrate the waters of the Rhone in one arm*," &c., &c.

And again, the official journal, "Annales des Ponts et Chaussées" (1863, 2d semestre), refers to this matter in these words (translated): "After having attempted to improve the mouth by concentrating all the water *in a single channel*, the administration" (*i. e.*, of the Ponts et Chaussées) "renounced their fruitless efforts and had recourse to the construction of a maritime canal," &c.

It seems scarcely necessary to remark that the jetty system has never been applied to the Rhone mouths. The method of closing useless passes having resulted in failure, the work was abandoned without even an attempt to apply the true jetty system, and the alternative project of Mr. Surrell—a "canal maritime" (ship-canal)—made. This, in the words of the report of the recent Board, "is more than adequate to the wants of commerce." Inasmuch as at that locality there are comparatively trifling wants to be supplied, in *our* sense of such needs, one might suspect a vein of irony in these words, when we consider what a "*canal-maritime*" would be to the wants of commerce of the port of New York. Even *those* wants should not rival the exigencies of the now enormous, but yet undeveloped, freight-commerce of the Great West through its only outlet where the Father of Waters bears to sea the spoils of half a continent!

A METHOD OF PRODUCING PURE CHARCOAL STEEL DIRECTLY FROM THE ORE.

By HENRY LARKIN.

From "Journal of the Society of Arts."

THERE is no subject in the whole circle of Metallurgical Art in which there exist such differences of theory, and such uncertainties in practice as exist in connection with the very ancient art of making steel. We are not yet agreed even as to the specific compound we shall indicate by that long familiar word. For thousands of years we have been handling steel, for good and for evil, and familiarly speaking of it as if we knew what we were talking about; and yet, even to this day, it remains a mystery. Iron we know, and gold and silver, and perhaps every other metal and alloy of metals in general use, so that, in selecting them, we can define with very great accuracy the precise material we desire to obtain. The value of a bit of gold or silver may, by careful assaying, be estimated with any degree of accuracy that may be desired. The value of a bar of iron, if it be simply iron, is a perfectly known quantity. But with steel it is not so. The moment we pass from the consideration of iron, or of any other simple metal or metals, to the consideration of steel, we pass from terms of scientific accuracy to mere customary phrases of eloquent empiricism. Fortunately, however, with such speculations we need not now trouble ourselves. To an ordinary user and worker of steel a bit of good steel means a material prepared from iron which he can forge into a tool, and so harden and temper as to produce a good working point, or edge, or surface. Until quite

recently the fundamental idea of all steel was tool steel, for piercing, for cutting, and for giving or resisting a blow. Within the last few years, however, a great change has taken place in our ideas on this subject. A new material has been introduced by Mr. Bessemer and by Messrs. Siemens-Martin of extraordinary value for strength and malleability, yet which is neither wrought iron nor tool steel, but intermediate between the two, both in composition and character; being as much stronger than the one, as it is milder and tougher than the other.

This mild form of cast steel, the manufacture of which has already grown to large dimensions, promises before long to supersede the use of iron for all the more important engineering purposes; and especially now that Sir Joseph Whitworth has perfected his method of casting under pressure, and thus of producing it as free from honeycomb, and as uniformly sound and reliable as the best wrought iron. But of this giant industry, full of interest for the future as it is, it is not now my business to speak. My present subject is one of far smaller dimensions, although by no means of smaller interest, namely, the good old fashioned tool steel, such as the natives of India knew how to make a thousand years ago, the value of which is not to be measured by mere thousands of tons. Really excellent tool steel may be a comparatively small thing in a commercial point of view, but it stands absolutely alone for value amongst all the implements and utilities of civilized life. We could better part with all the metals ever dug out of the earth than we could part with the steel from which our tools are made. There is not an art, or a comfort, or hardly a mouthful of food that we eat but, in one way or another, is dependent on steel. A good, trustworthy bit of tool steel is simply invaluable. But what shall we say of a bad bit of steel in comparison? There is *no* comparison. The difference is that of opposites; like that of good and evil, beneficence and malfeasance, like that of trustworthiness and of treachery. There is nothing that man can make more trustworthy than steel, and there is nothing more disastrously treacherous. Man has been wisely defined as a Tool-using Animal. With the exception of a few microscopic rivals and forestallers of our privilege, which Mr. Darwin knows of—Man alone knows how to make tools and how to use them. The highest artist is simply the man who can handle the best tools to the best purpose. The perfection of our tools is but another name for the perfection of our material civilization, and every tool we can lay our hands to is made either of steel or by steel.

Such being the importance of steel, one would have thought among all the developments and applications of modern science, one of the first and most indispensable would have been, to know how to make good steel with the same certainty and uniformity as we know how to make good sovereigns and shillings. But instead of this being the case, I believe I am stating a mere well known fact, when I express my belief that there is not a manufacturer in Sheffield who really knows why his steel comes out sometimes good and sometimes bad. The bulk of our tool steel is still made, not by the light of science, with clear understanding of every step that is taken, but by the merest technical routine and rule of thumb.

Some three or four years ago I was ambitious enough to hope that I might do some little to correct this anomaly. It seemed to me that the production of steel must necessarily be an uncertain thing so long as the ordinary routine of doing and undoing—of smelting, and puddling, and converting, and sorting by rule of thumb was concerned. While occupied with these speculations, I was so fortunate as to obtain a few tons of the well known magnetic iron sand to experiment with; and my thoughts were —Here is a perfectly uniform material, free from impurity that would be injurious to the steel which might be made from it. If I can succeed in converting this material into steel by the agency of pure carbon and by strictly uniform treatment, I ought to succeed in producing a pure and uniform steel. My notion was, to thoroughly mingle the powdered ore with powdered charcoal in accurately regulated proportions, and then to reduce it in closed retorts at the lowest practicable heat, so as to prevent the reduced metal from cohering to-

gether. I saw from the first that if I could only obtain the reduced metal in a pure and powdered condition, I should have no difficulty in mixing together any reasonable bulk of such powder, so as to be uniform throughout; and that I could, by analysis or other experimental test, ascertain accurately what the entire bulk consisted of, and so know perfectly the material I was to work on. The next step I proposed was to prepare the material so obtained for being melted into cast steel of any required quality, by adding to, and intimately mingling with it just so much carbonaceous material as should be found necessary. This uniform mixture would only then need to be uniformly melted and worked; and, so long as the operations of nature are uniform, the result obtained could be no less so. Such was the origin of my method for making pure charcoal steel directly from the ore: which method, after the usual experiences of long months of disaster and disappointment, I have now the great privilege of explaining, in its matured and I modestly trust triumphant condition.

Before, however, proceeding any further with this explanation, it would probably be of some interest, and certainly will be only fair to my less fortunate predecessors, if I give a short sketch of what, almost unknown to me at the time I speak of, others have done or have proposed to do towards the solution of this interesting metallurgical problem. In order to keep any subject within reasonable limits, I will confine my notice strictly to the various attempts which have had for their leading principle the reduction of iron ores at a comparatively low heat in closed retorts. It has long been familiarly known that iron ores could be reduced at a heat very much lower than that needed for the fusion of the metal. In fact cast iron and cast steel are quite modern inventions. Originally all iron and steel was produced at such comparatively low temperatures, and worked only at a welding heat. But the idea of reducing iron ores at a low heat in closed retorts belongs, I believe, entirely to the present century.

So far as the records of the Patent-office inform us, Mr. William Neale Clay seems to have been the pioneer and first actual worker in this enterprise. In the year 1837 he obtained a patent for "Improvements in the manufacture of Iron." In his specification he speaks of the difficulty then experienced in smelting the richer kinds of hematite ores, which could only be done by "mixing them with poorer ores at considerable cost." He therefore proposes a method for "working such rich ores, and producing malleable iron therefrom by a very simple process, and at a comparatively small cost." He says:—"I take any quantity of red Lancashire or Cumberland ore, or other ores of a rich character, and break the larger lumps, by means of a pair of rollers or otherwise, to about the size of walnuts, which I believe to be the best size for working. With one hundred parts by weight of such broken ore, I mix twenty parts of clean dry coal ashes, or cinders, or of coke, charcoal, charred peat, anthracite coal, or other suitable carbonaceous matter, broken so as to pass through a sieve of half-inch mesh. The mixture is put into retorts or vessels, which I prefer to be of a ⌂ shape, about seven feet long, and eighteen inches high, and two feet wide, made of clay, fire-bricks, iron, or other suitable material." There is no special arrangement for heating the retorts, beyond causing the waste heat from a puddling or other furnace to pass through flues carried transversely beneath a row of such retorts, placed side by side, seemingly, from the drawing given, to the fabulous extent of some fifteen or more. It is quite certain that no such extensive arrangement of retorts was ever actually worked on this plan; but it is no less certain that Mr. Clay did really obtain a remarkable degree of success by his very simple, although crude arrangement. His retorts are shown as opening only at one end. Through this opening the mixture of broken ore and carbonaceous materials was shoveled into the retort, which when thus filled was closed by means of a movable door. After the mixture had been kept at a full red heat for a sufficient number of hours to insure the reduction of the ore, the door of the retort was removed, and the charge withdrawn into an empty iron barrow run alongside to receive it. It does not appear what precaution was taken to prevent the expos-

ure of the red hot reduced material to the oxygen of the atmosphere. We are only told that when malleable iron is required, "the charge on being withdrawn from the retorts may be immediately conveyed into a puddling or balling furnace." The completion of the reduction was to be discovered "by taking out some of the pieces of ore from the retorts by means of a pair of tongs, and with a file filing the surface of such ore to ascertain whether it had arrived at a metallic state."

No further patented attempt of any note seems to have been made towards the solution of our problem until 1854, nearly seventeen years later. In that year a patent was obtained by M. Chenot, the scope of which was very much wider and more ambitious. He proposes to make what he calls "metallic sponges" from rich iron ores by reducing them in closed furnaces by the agency of pure carbonic oxyde. This pure carbonic oxyde is to be expressly manufactured for the purpose in the first instance. It is then used for the reduction of the ore, during which operation it takes up another atom of oxygen, and becomes converted into carbonic acid. This carbonic acid is carefully collected as it passes from the reducing chamber; and a sufficient portion of it is reconverted into carbonic oxyde by causing it to pass through "a retort containing carbonized matter at an incandescent or red heat." By this means he says: "I obtain a rich source of pure carbonic oxyde; and the use of gas, which has hitherto been impracticable, is made very easy and effectual." If, however, the ore from which the "metallic sponges" are thus made should not be of sufficient purity to produce metal of the quality desired, "the sponge is to be pulverized by any suitable apparatus, and after pulverization the matter passed to an electric sorting machine." No intelligible working description is given of this "electric sorting machine;" and the only novelty claimed for it is "the use of electro-magnets, employed to sort in a continuous manner," instead of "natural or permanent magnets, which have been used on a small scale to free brass filings from particles of iron." Nevertheless, the idea of applying it to such a purpose is of undoubted value, and M. Chenot is fairly entitled to the credit of having proposed it. Another idea of at least equal value is that, after the "metallic sponge" has been pulverized, and thus freed from extraneous matter, the metallic powder may, if necessary, be intimately mixed with carbonaceous matter and compressed into any desired shape. It is very singular that the value of this idea of mixing powders was so little appreciated that it is actually added as a kind of second best expedient. It seems not to have occurred to the writer that when he had thus got his materials into a powdered condition he could mingle them as accurately and as uniformly, as if they were so many liquids. His main idea runs entirely on the singular advantages of the spongy condition. He says that if the ore be sufficiently pure "it need not be pulverized, and as the sponge properly prepared is porous, that property is used thoroughly to incorporate with the sponge the carbon necessary to the constitution of steel. To this effect the sponge is plunged into a fatty preparation, whose composition is calculated according to the density or absorptive power of the sponge, which latter absorbs equally throughout, and the excess of fat is driven off by distillation almost carried to carbonization, and the sponge then contains all the constitutive parts of the quality of steel desired, the hardness of this steel varying according to the density of the fatty preparation employed. This method constitutes an entirely new principle, by means of which I obtain carbonate of iron with exact doses of carbon incorporated throughout its mass without the use of heat.' There are many other ideas put forth in this specification which I need not specially refer to. The two points which seem to me to be really interesting, or at least which in any way anticipate my own method, are those which I have mentioned, namely, the "electric sorting machine" and the occasional pulverization of the "metallic sponge" and its subsequent compression in moulds. There is no detailed description of any furnace or other apparatus by which the ideas are carrried into practical effect, and there is very little that gives one the conviction of its having been writ ten from actual working experience. In all cases, "metallic sponge" is M. Che-

not's leading thought; and he only proposes to pulverize it when it proves otherwise unmanageable. This metallic sponge he proposes to melt into steel, either in a cupola, stratified with carbonaceous material, or in crucibles, or in the open hearth of a reverberatory furnace.

The next patent bearing directly on our subject is dated only a few weeks after M. Chenot's. This is also by a French gentleman, M. Bellford. The specification describes a very elaborately arranged furnace, in which reducing, refining, and melting are all carried on in a continuous series of operations, without allowing the material to cool down in the process. But the entire arrangement is ingeniously hypothetical and quite impracticable. The one point of interest for us is that "the ore from which the steel or wrought iron is to be produced is first pulverized in a sufficient manner according to its general character, and next intimately mixed with charcoal or other sufficiently carbonaceous matter."

After this, curiously enough, there is another interval of seventeen years before the records of patents show any renewed activity in our special direction. In 1871, Mr. T. S. Blair, of Pittsburgh, Pennsylvania, obtained a patent for "Improvements in the means and apparatus for the reduction of Iron Ores, and for preparing the same for reduction." The specification describes very clearly two ingenious and perfectly distinct arrangements of apparatus for exposing finely pulverized iron ores to currents of heated reducing gas. Nothing is said as to how the spongy iron thus produced is to be utilized; and I am unable to say whether either arrangement has stood the test of actual trial. But the probability is negative, as in 1872 Mr. Blair again comes forward with a patent for "Improvements in the manufacture of Iron and Steel, and apparatus therefor," the apparatus therefor consisting of a third arrangement of reducing furnace, much more elaborate than the two previous arrangements, and altogether different. This last improvement consists essentially of an arrangement of upright cylindrical retorts, about thirty feet high, and three feet internal diameter. The ores is to be fed in with carbonaceous matter at the top, and, when reduced, to discharge itself by its own weight, cold, from the bottom: the reduction being continuous, and the retorts kept always full. This idea of continuous reduction in upright retorts is rather taking in its simplicity, and was already adopted by M. Bellford in his special apparatus; but there are serious objections to it which will be considered presently. In this third arrangement of furnace Mr. Blair proposes to use as his reducing agent, either solid carbonaceous matter, or "gas flames rich in carbon." But, he adds, "when the carbon is supplied in a solid state (as charcoal, coke, &c.), the mixture to be treated will be found a bad conductor of heat, and if the process were conducted in a chamber of large area it would be impossible to convey to the central portion the necessary heat, so as to perform the work with regularity all through the mass. Hitherto this difficulty has not been successfully met. My improved method, which may be called 'initial heating,' removes the difficulty altogether."

If this were true, Mr. Blair's triumph would be great indeed. But this notion of the importance of "initial heating" is based on a very evident fallacy, the fact being that the real pinch of the work lies in the final, not at all in the initial heat given to it. Mr. Blair has devised what appears to be a sufficiently effective arrangement for bringing the upper part of his furnace to a full reducing heat, and he seems to think that having once thus started the operation, he has only to let the material slip down into the body of his furnace with a very moderate supply of extraneous heat, when the process of reduction will continue of itself! I read many months ago, in an American periodical giving an account of the exceeding economy of Mr. Blair's process, a distinct statement to this effect, namely, that the very operation of reduction, when once started, generated sufficient heat to secure its continuance, if only the inevitable loss by conduction and radiation were compensated by some small external supply. If this were in accordance with the facts, I should have to congratulate Mr. Blair on the efficiency of his furnace arrangements; but unfortunately the fact is ex-

actly the reverse. The act of reduction, whether performed at a red heat or at a melting heat, absorbs heat instead of generating it. In fact it is a cooling operation, and not a heating operation at all. It is true the carbon consumed is half burnt into carbonic oxyde, and a certain equivalent of heat is thus set free; but simultaneously the oxyde of iron has to be wholly *un*burned, and a certain proportion of heat must as inevitably disappear. It is to supply this constant absorption during the whole operation of reduction, and, as it were, to compel the oxygen to leave the iron and put up with the carbon, that the heat of the furnace, so long as the reaction is to continue, needs to be kept up to its full efficiency by an extraneous supply. The notion of this process of reduction of iron when once fairly started, going on pretty much of its own accord, is, alas, but another instance of the "perpetual motion" futility, that brightest hope of the unregenerate human heart, to persuade nothing to bring forth something!

I have no hesitation in saying that any possible improvement in the "initial heating" of our materials, instead of removing the difficulty altogether, would be a matter of little or no importance to the success or the economy of the operation. The initial heat may be a very dull red, without involving much loss of time. The first half of the oxygen is very easily and quickly got quit of. The iron at first seems ready enough to part with it; but it is very different when it comes to its last gasp. Then comes the real tug of war between iron and carbon; and nothing short of full red heat, with clear preponderance of carbon, would ever compel the iron to quit its final hold.

I said there were serious objections to upright retorts for reducing. I do not pretend to imply that these objections cannot be fairly met. But to give my objectors every chance, I will briefly state my experience; for I have tried both methods. In the first place, I have not found the force of gravity to be at all sufficient to be depended on to clear them effectually. If the heat be raised a little too high, portions of the reduced metal are constantly liable to stick to the inside of the retort, and need to be thoroughly cleared off, or they would become more and more firmly adherent, until they formed a very serious obstruction. With a horizontal retort, in which each charge is entirely removed before another is introduced, this presents but little difficulty. But with a perpendicular retort, acting continuously and always kept full, it seems to me the difficulty would be insuperable. And Mr. Blair himself ominously admits that "the heat should be carefully kept at the required degree, which is a fair red tending toward yellow, as a higher heat makes the sponge sticky, and gives rise to trouble."

In the next place, even if the material did not stick very tightly to the retort, but the heat were only so much in excess as to cause the material to cohere together but slightly, and this is a thing which would be sure to happen occasionally, the thoroughfare would be inevitably blocked, and the furnace would cease to be self-acting. Lastly, even if the whole contents of the upright retort were periodically cleared out, as in the case of the horizontal retort, it seems to me, as an eye-witness of the difference, that the punishment the workmen would experience in standing over the mouth of a red hot retort to clean it, compared with that of standing at a moderate distance in front, would be alone sufficient to decide the question of their respective efficiency. Of course, in judging of what I now say, every needful allowance will be made for any possible bias of a rival patentee; but I am unconscious of having said one word from a wish to disparage any just claims on the part of Mr. Blair. I fully admit the great ingenuity of his inventions; but I do not believe in their practical value, and if I am to speak of them at all, I am bound to say so frankly. Mr. Blair has a further patent, two years later, in which he claims "the producing cast steel in the open-hearth process by the treatment of iron sponge with admixture of carbon and agents for accelerating carbonization," instancing "yellow prussiate of potash." But in this idea he was anticipated twenty years before by M. Chenot, who, in describing his methods of carbonizing iron sponge for the production of cast steel, either in crucibles or on the open hearth, says: "the carbon may be

found in any powdered carbonaceous matter, or derived from salts of iron, of manganese, or of other metals, from alkalies in the state of cyanides, or from cast iron."

We must now return to the year 1872, in which year our American cousins seem to have been especially alive to the importance of the problem now under consideration. A few weeks after the date of Mr. Blair's principal patent, a patent was obtained by Mr. Joel Wilson, for "Improvements in Furnaces for deoxydizing Iron Ores preparatory to their being worked into Wrought Iron." The specification describes a circular arrangement of upright retorts for the continuous reduction of the mixed materials, the main object of the invention being a more efficient and economical method of heating the same. In the same year a patent was obtained by Mr. T. R. Scowden, also of the United States, for "Improvements in the manufacture of Steel, and in apparatus employed for this purpose." This invention relates to "apparatus by means of which articles of iron have their exterior portions or entire substances converted into steel, by treatment with hydrocarbon vapor, and heat;" and consists further in the application of the same arrangement to the conversion of spongy iron into spongy steel.

In 1874 a patent was obtained by Mr. N. W. Wheeler, of the United States, for "Improvements in the art or process of reducing iron and other ores, the production of Steel, and in apparatus for the practice of the same." According to this invention, "ore in form of sand or powder is let fall in a shower down a shaft, having the upper part filled with a column of heating and even oxydizing flame, and the lower part filled with a column of reducing gases, so that the particles fall first through the flame, and during such fall become heated to fusion or incandescence; and afterwards entering and falling through the reducing gas, yield up their oxygen to the carbon or hydrogen, and thus fall in a reduced state into a hearth or crucible at the bottom of the shaft."

Two days later a patent was obtained by Mr. Edgar Peckham, of the Uuited States, for quite an assortment of improved reducing furnaces. On this elaborate specification the only concise account I can give is, that it consists of fifteen pages of printed matter, and is illustrated by twenty-four very carefully prepared drawings. For further particulars I must refer to the document itself, for I can give no sufficiently brief description that would be of any value of the several arrangements proposed.

I have now indicated as concisely and as faithfully as I am able all the various proposals bearing on our subject which have been patented in England. And I believe I have frankly stated all the points in which I found that my own ideas have been anticipated. But I must add, not one of the specifications referred to shows any recognition of the facilities afforded by the powdered condition of the materials for accurately mixing, and for obtaining precision and uniformity of result. And, accordingly, not one of them proposes to begin with for reducing, a uniform mixture of powdered ore and carbon; and to end with for melting, a mixture of uniform metallic powder, with accurate proportions of whatever other powdered material may be necessary to produce the quality of steel required.

The list which I have gathered is the result of several days' very close research at the Patent office, and I trust that no patent of any real importance has escaped my notice. No one of course is bound to take my judgment on the several proposals as in any way final. I have in each case given both name and date when referring to a patent, and it will be a comparatively easy task for any person desirous of pursuing the matter to recur to the original specifications and judge for himself, For my own poor judgment, I must say they nearly all seem to bear the marks of having been chiefly, and in some cases entirely, worked out on paper, instead of through the disastrous yet wholesome discipline of actual experience. With the single exception of Mr. Clay's patent, taken out nearly forty years ago, I doubt whether one of the arrangements so elaborately described on paper has stood the test of six months' good work actually done. M. Chenot's specification is, for the value of his suggestions, worth all that I have ever read on this subject. But his proposals, valuable as

they are as suggestions, are very far from being carried out into working detail; and it is hardly to be wondered at, that a mind which could aim simultaneously at so many good things, should have practically missed them all. I believe the one secret of success in all such unknown enterprises is, to stick pertinaciously to one thing at a time, until you have thoroughly conquered it, and got it fairly in hand. I should be sorry to weary any one with a tedious account of all the difficulties with which I have had to struggle, one by one, in maturing the idea with which I started. That idea I have alread indicated. And in that simple idea I venture to hope I have laid the foundations of a new method of producing steel, by which that seemingly inscrutible metalloid will yet be brought under the control of science, and be obtained in all its useful varieties with clear-eyed accuracy and certainty.

I trust we shall once more be able to produce steel of the purest and very highest character, rivalling even the Damascus and Toledo blades of which history speaks with such enthusiasm, but which, to most modern manufacturers, have become an unimaginable and incredible myth. There was a time when a man could speak of his "trusty sword" as an almost sacred thing, which, while strength remained in his own right hand to wield it, could never fail him; when the proverb, "True as Steel," went home to each man's conscience, indicating to him whatever was most to be depended on in the hour of trial and direst need. The time now is when the high character has almost passed away; when a new proverb, "Treacherous as Steel," would more fitly express the experience of those who have to deal with it. I make bold to hope that we are fortunately destined, sooner or later, to change all that; that we shall all gradually rise to a higher appreciation of the value, not merely of really good steel, but of really good quality in all things; that "True as Steel" will not always continue the unmeaning phrase it now is, but will once more become the expressive symbol of all that is most honorable in our dealings with each other; and that we shall yet practically find, in all that we are engaged in, that business and beneficence may really walk hand in hand together, to the quite infinite advantage of both.

I am very far from boasting that we have already altogether realized this sanguine dream. I know too well how easily the best steel may be spoiled by unskillful manipulation, either in the melting or in its subsequent treatment, to hope that such perfection of results will ever be attained without a corresponding perfection of care and experience in every step of the process. I have no hope of ever being able to dispense with human faithfulness and sagacity; and I cannot confess to even a wish to do so. My utmost thought is to give faithfulness and sagacity a friutful field to work in; and an intelligible method of working, by which good work shall necessarily lead to an equally good result. And this I do claim to have actually done. The Red Moss Metal Company has been working the process on a small, but regular manufacturing scale in Warrington for some eight or nine months, with all the difficulties of inexperience to contend with; and the tool steel which they have produced, for all varieties of temper and purposes, has been pronounced by those who have used it, with but few exceptions, superior to any other steel obtainable: samples of all which are now lying on the table.

It is true that we have made our mistakes and had our disappointments, owing chiefly to comparative inexperience in the melting; but when I say that we do not go in for picked steel, and that the high testimonials we have received are simply testimonials to our average production; and that it costs us no more to produce the best steel than it would cost us to produce the worst; manufacturers of steel will fully understand the importance of such a statement, however little they may as yet be able to credit it.

I said that I began my experiments with a few tons of pure magnetic iron sand. There are many large deposits of this singular ore in various parts of the world. One of these deposits, many miles in extent, exists on the coast of Taranaki, in the northern island of New Zealand; and a few years ago there was a great talk of the wonderful iron and steel that could be made from it. But all these sands, which are essentially disin-

tegrated magnetic oxyde of iron, although specially suited to the process under consideration, are very troublesome to work in a smelting furnace, on account of their liability to choke it. For this reason there has been hitherto little or no market for it, and there has been no encouragement to any one to bring a continuous supply. In consequence, we have not found it prudent to depend on any mere iron sand as the basis of our manufacture. Any rich and pure ore is almost equally available; although there is an advantage in the magnetic oxydes which will appear as we proceed. The ore which we are using comes from Marbella, on the south coast of Spain. It is a magnetic oxyde, rich, pure, and easily disintegrated, and it can be obtained through the Marbella Ore Company regularly, punctually, and in any quantity required.

The large and small lumps of this ore are first passed through the jaws of a Blake's crusher, set as closely together at the bottom as practicable, and the crushed material is sifted as it falls. The coarser portion is then passed through a disintegrator. In this way the whole bulk of the ore is very cheaply and readily reduced to the condition of the iron sand already described. But of course the gangue of the ore is crushed equally with the ore itself; and the next step is to separate the actual ore from all such extraneous matter, and get as nearly as possible a pure oxyde of iron. This is very effectually done by means of a self-acting magnetic separating machine, specially devised for the purpose, and capable of dealing with large quantities of material. In this machine the particles of magnetic oxyde are picked up by magnetic attraction, and carried into their proper receptacle while the refuse is safely deposited in another. Having thus got as pure and rich a material as possible in a powdered condition the next operation is to thoroughly mix with it a sufficient quantity of powdered carbonaceous matter to combine with the oxygen of the ore, and thus effect its reduction. The carbonaceous matter used consists of powdered charcoal and powdered resin, or other suitable bituminous substance, the two being reckoned together somewhat in excess of the oxygen to be removed. This mixture of powdered ore and carbonaceous powders is slightly warmed, and compressed into bricks in an ordinary brick press, and will then be ready for the reducing furnace.

The reducing furnace consists of a series of ⌂ shaped gas retorts, with doors to open at each end. These retorts are heated by a fire acting somewhat upon the principle of a Siemens' gas-producer, and are thoroughly supported throughout their entire length by an intricate arrangement of brickwork, which also serves to prevent a too ready escape of hot air into the flue. The burning gases from the fire are also made to completely envelope the retorts by being carried over and under in a zigzag way, thus still further delaying their passage and arresting the heat with which they are charged. Air-holes are opened at regular intervals in order to complete the combustion of the gases as they circulate around the retorts, thus securing the greatest heat where it is actually wanted and also securing complete combustion of the fuel used. I need hardly add that the consumption of the smoke is perfect. There are other points of importance in connection with the furnace, but as they would not help to a clear idea of the method as a whole they may be omitted.

Let us now imagine one of these retorts at an average working heat, empty, and ready to be charged. The door being removed from the feeding end of the retort, a small stack of pressed bricks, consisting of ore and carbonaceous matter, and of bulk to fill the section of the retort, is closely packed on a rectangular plate, and pushed into the further end by means of an iron rod. The iron plate is then withdrawn, leaving the small stack of bricks securely placed. A second and third feed immediately follow, filling the retort, which is at once closed. After having been exposed to a pretty full red heat for nearly twenty-four hours, gas will have ceased to be given out, the carbonaceous matter will have become practically consumed, and the oxyde of iron will have become converted into red-hot iron powder.

The next problem is how to convey this red-hot powder from the retort without exposure to the action of the atmospheric air, and to keep it so till it

is cold. We will now imagine a charge ready for such removal.

Ordinary coal gas is first, by means of pipes provided for the purpose, turned on into the inside of the discharging end of the retort, in order to produce a full outward pressure of gas while the discharging door is removed, the door being at the under side of a projecting end-piece of the retort. This door being thus removed, an iron receiver is brought up closely under the projecting end-piece, and securely supported there. By a similar arrangement of pipes, gas is now let also into the inside of the feeding end of the retort, when the door of that end is quickly removed, and a temporary door with a wide slot halfway down the middle of it, is put in its place. The slot is for the introduction and working of the discharging tools, by which the red hot powder is quickly pushed forward into the receiver placed at the discharging end. As soon as the retort is empty, the gas at both ends is turned off, and the iron receiver containing the metallic powder is removed and kept carefully closed until its contents are cold. When the metallic powder is sufficiently cooled down, and no injury can arise from its exposure, it is turned out of the receiver, and again passed through the disintegrator and the magnetic machine for a final purification. Thus by a few simple and almost self-acting operations, requiring little more than faithful attention and accuracy in weighing and mixing, we are able to produce the pure metallic powder, the value of which I have already sufficiently urged. For the production of tool steel, which is the subject immediately before us, we mix with the metallic powder (besides some small percentage of flux) whatever additional amount of carbon may be needed, chiefly in the form of resin. This resin enables us easily to compress the finished powder into solid cakes, in the same way as the bricks of ore and charcoal were compressed in the first instance. The cakes of finished material are then stacked up, ready to be melted in crucibles in the usual way, with the addition of manganese or any other alloy that may be found advantageous.

If I were now asked for a definition of steel, which should be quite up to the latest development of science and art, instead of venturing upon some high speculative abstraction, I am afraid I should be tempted to modestly reply :—A metalloid obtained from an accurately adjusted mixture of pure iron powder and carbon, perfectly fused with the addition of manganese, either in crucibles or in an open hearth furnace.

PROFILES OF HIGH MASONRY DAMS.

By JOHN B. McMASTER, C. E.

Written for VAN NOSTRAND'S MAGAZINE.

I.

THE subject proposed for consideration in the following paper is that of the profile of masonry dams of such height, breadth and general dimensions as would be required for reservoir purposes, or for impounding the waters of rivers and large streams for mill or irrigation use. We would observe, however, at the outset, that as this matter has already been treated with such fullness by several writers, and especially by MM. Delocre and Sazilly—to whose excellent "memoirs" we are greatly indebted—we can hope to add little that is really new, but shall endeavor, by drawing from many sources, to supply our own deficiency, to diminish the errors of others, and thus obtain results very much more accurate than could be derived if we relied solely on ourselves.

Before, however, we take up the consideration of the matter of the form of profile that shall combine the greatest strength with the least amount of material, there are a number of important points to be considered somewhat in detail. Thus, it is necessary, in the first place, that we should know the forces to

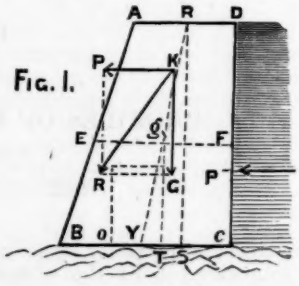
which dams are subjected, their kind, whether constant or variable, the methods of determining their direction and calculating their intensity, and the effects they are likely to produce, and these matters being known, we may pass to the consideration of the conditions of stability, first when the dam has only its own weight to support, and, secondly, when it has to withstand both its own weight and the pressure of the water. We may then deduce a theoretical profile of equal resistance, and, finally, adopt one so modified by the requirements of practice and suggestion of experience, that it shall serve as a *profile type*, fulfilling to the utmost the requirements of great strength and stability, beauty of outline and economy of material.

Now, it becomes evident, after a moment's consideration, that there are but two forces that may at any time be regarded as acting with vigor on a dam, and these are, the weight of the masonry, cement and other material composing the structure, and the pressure or thrust of the water whose flow it checks. The first becomes, to all intents and purposes, a constant quantity as soon as the dam is finished, and continues so for ever after, acting vertically downwards through the centre of gravity of the mass. But, on the other hand, the latter force is one of great variability. For, as its intensity at any moment depends on the depth or head of water behind the dam, increasing as the water deepens and decreasing as the water falls, and the head of water, especially in reservoirs used for mill or irrigation purposes, being subject to frequent rise and fall, it follows that this thrust must be considered as a variable quantity and treated accordingly. It is, moreover, to be observed, that this thrust acts horizontally, and unlike the weight, is not distributed uniformly over the entire face of the dam, being almost, if not quite, zero at the point where the water cuts the masonry, and growing greater and greater as we descend towards the foot of the dam. The weight, it is true, also increases as we go from the top to the bottom, yet, if we suppose the dam to be at any point ten feet thick, the pressure on any horizontal section taken at that point will be everywhere the same, and this is by no means the case if we take an area ten feet square on the water face of the dam, and against which the fluid presses.

In order that the dam may not yield under the first force, and be thrown down by the greatness of its own weight, it is necessary, should the structure be of such height, or the material of such heaviness, that the pressure per unit of surface at any horizontal section is in excess of the "limit of pressure" for masonry, that the surface of the section be increased so that the pressure being distributed over a more extended area the load at each unit of surface shall be less. The second force, or thrust of the water, is resisted at any point by the weight of the masonry above that point, and by the friction of the stones, which is of course dependent on the weight. Some resistance is indeed afforded by the bonding power of the hydraulic mortar used in setting the stones, but this is so small that precautions of safety require that it shall in all calculations be disregarded entirely.

But these two forces, the weight acting vertically downwards and the thrust of the water acting horizontally, counteract each other to a certain extent, and give rise to a third power or resultant, the position of which, as regards the base, will determine the stability of the dam. To illustrate, let A B C D (Fig. 1) repre-

FIG. 1.



sent the profile of a dam composed of horizontal courses of masonry bedded on each other, and K the centre of gravity of the mass, lying above the line E F. Represent by K G the direction and intensity of the weight of A F, and by K P the direction and intensity of the thrust of the water from D to F. Then, constructing in the usual way the parallelogram P K G R, we shall have for the resultant of K P and K G, the line K R.

Now, supposing the dam to be perfectly secure as to its weight, the force P of the water can demolish the wall only, when, exceeding the weight and friction K G, it shoves the mass A F along the joint E F, or causes it to rotate about an axis through E. Which of these motions, the slipping or rotating, shall take place depends entirely on the magnitude and direction of the resultant K R. If the pressure of the water is so large compared with the weight that the angle R K G, which the resultant makes with the vertical, is larger than the angle of friction (32° for masonry on masonry), the mass A F will then *slide* along the line E F; while if the position of the resultant is such that it passes without the base B C, then rotation will take place about the axis of E. Of these two motions, the latter is in practice the most likely to occur, inasmuch as in nine cases out of ten when rotation does take place it does so about some point as E′, nearer the resultant than E, because the pressure concentrated at E, breaks off the stone, and thus throws the axis of rotation nearer the resultant.

The condition of stability then, in dams that do not transmit laterally to the sides of the valley, the pressure they sustain (and this is the case in all large dams) is, that they must resist this pressure at every point by their own weight. If the material employed were of considerable resisting power, as well as the soil of the foundation, and if there were between them an unlimited degree of adhesion, the only condition of stability to be fulfilled would be, as we have just seen, to give the wall such a profile that the resultant of the thrust of the water and the weight of the dam shall pass within the polygon of the base. But this condition is not found sufficient in practice; the material and the soil of the foundation will, in fact, support only a limited pressure (depending on their nature), and they have not between them an unlimited degree of adhesion. Hence, the two following indispensable conditions:

1° The several courses of masonry in the wall must be incapable of slipping the one over the other, and the wall incapable of sliding on its base.

2° In no point of the structure may the material employed, or the soil of the foundation be required to bear too great a pressure. To begin with the first condition.

STABILITY AS TO SLIPPING.

We shall take up first the condition of stability as to the slipping of the various courses of masonry, and then pass to that of the entire dam. The first thing to be now determined, is the horizontal thrust of the water. Suppose A B C D (Fig. 2) to represent the face of

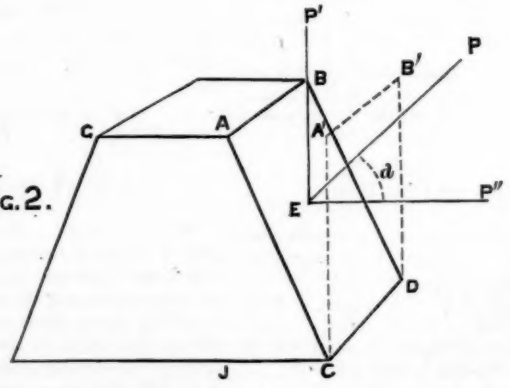


FIG. 2.

a dam pressed by water, and let h=A J denote the height; a=J C the projection of the slope of the dam on the horizontal plane; and, finally, let l=A B denote the length of the dam, and b=A G is breadth across the top. Then will the *vertical* pressure of the water on the face A B C D be expressed by

$$a\,l\,\frac{h}{2}\,y=\tfrac{1}{2}\,a\,l\,h\,y \quad . \quad . \quad . \quad 1$$

and the horizontal *thrust* by the expression

$$l\,h\,\frac{h}{2}\,y=\tfrac{1}{2}\,h^2\,l\,y \quad . \quad . \quad . \quad 2$$

in each of which y denotes the density of the water. These equations are obtained as follows :

Let E P, in Fig. 2, represent the normal pressure of the water on the surface A C, which we will call F, and resolve it into two components, one vertical E P′, and one horizontal E P″, and call them respectively P′ and P″. Then expressing the angle P E P″ made by the horizontal component P″ and the normal E P, by a we shall have from the triangle E P P″

$$\frac{P\,P''}{E\,P}=\sin P\,E\,P'' \text{ or } \sin a.$$

But P P″ = E P′ = P′, hence

$$\left.\begin{array}{l}\dfrac{P'}{P}=\sin a \quad \text{or } P'=P\sin a.\\ \text{In the same way we find}\\ \dfrac{P''}{P}=\cos a \quad \text{or } P''=P\cos a.\end{array}\right\} \quad . \quad . \quad 3$$

Now, let a projection A′ B′ C D, of the surface A B C D, be made on a plane at right angles to P″, and call the area of the projected surface F′. Then will F′ = F cos A C A′, or since the angle of inclination A C A′ of the surface to its projection is equal to the angle P E P″ = a, between the normal to A C, and the perpendicular to A′ C, we shall have $F'=F\cos a$ or $\cos a=\frac{F'}{F}$. But cos a is by equation 3 equal to $\frac{P''}{P}$, and therefore,

$$\frac{P''}{P}=\frac{F'}{F} \quad \text{or } P''=P\,\frac{F'}{F} \quad . \quad . \quad 4.$$

From the principles of mechanics, we know that the pressure P of water on any given area is the product of the area, the height h of the water, and its density y, so that in the present instance F being the area of the surface A B C D, we shall have for the value of P the expression $P=F\,h\,y$, and this substituted in equation 4 gives

$$P''=F\,h\,y\,\frac{F'}{F} \text{ or } F'\,h\,y. \quad . \quad . \quad 5.$$

Therefore is the pressure with which water presses against a surface in a given direction equal to the weight of a column of water, which has for its base the projection of the surface pressed, and for height the depth of the centre of gravity of the surface below the top of the water. We see, moreover, from the above, that since the projection at right angles to the vertical is the horizontal, and the projection at right angles to the horizontal is the vertical projection, the vertical component of the pressure of water against a surface may be found if the *horizontal projection*, or its *trace*, be considered as the surface pressed, and, on the other hand, the horizontal component may be found if the *vertical projection* of the surface, or its *trace*, be considered as the surface pressed.

Applying these two principles to the case of Fig. 2, and replacing F′ in equation 5, by its value $l\,h$, we shall have for the horizontal thrust of the water on the face A B C D of the dam the equation $P''=\frac{1}{2}\,h^2\,l\,y$, and in the same way the vertical component will be found to be equal to $P'=\frac{1}{2}\,a\,h\,l\,y$. Now, b being the breadth of the dam, and a' the projection of the slope G K, and y' the density of the masonry composing the dam, it is evident that the area of K C E G will be $\left(b+\frac{a+a'}{2}\right)h$; the cubic contents $\left(b+\frac{a+a'}{2}\right)hl$, and the *weight* $\left(b+\frac{a+a'}{2}\right)h\,l\,y'$. The whole vertical pressure on the base will therefore be equal to this weight *plus* the vertical pressure of the water, or

$$\tfrac{1}{2}\,a\,l\,h\,y+\left(b+\frac{a+a'}{2}\right)h\,l\,y'$$
$$=\left\{\tfrac{1}{2}\,a\,y+\left(b+\frac{a+a'}{2}\right)y'\right\}\,h\,l\,. \quad 6.$$

We have seen, however, that the force which tends to counteract the push of the water, and on which the stability as to slipping must therefore depend, is equal to this weight of the dam increased by the *friction* of the stones. Denoting this co-efficient of friction by f, we shall then have for the force to push the dam forward the expression

$$\left\{\tfrac{1}{2}\,a\,y+\left(b+\frac{a+a'}{2}\right)y'\right\}f\,h\,l$$

and in the case where the horizontal thrust of the water is to effect the displacement

$$\tfrac{1}{2}h^2 l y + \left\{ \tfrac{1}{2} a y + \left(b + \frac{a+a'}{2} \right) y' \right\} f h l$$

or dividing each member through by $\frac{1}{2} h l y$, we shall have

$$h = f \left\{ a + \left(b + \frac{a+a'}{2} \right) \frac{y'}{y} \right\} \quad . \quad 7.$$

In order therefore that the dam may not be pushed away by the water, we must have one of the two following conditions fulfilled; either

$$h < f \left\{ a + (2b + a + a') \frac{y'}{y} \right\}$$

$$\text{or } b > \tfrac{1}{2} \left\{ \left(\frac{h}{f} - a \right) \frac{y'}{y} - (a + a') \right\} \quad 8.$$

For safety, we may further assume that the base of the dam is quite permeable, in which case there is (on the principle that a pressure in one direction produces an equal pressure in the opposite direction) a pressure from *below* upwards equal to $(2b + a + a') l h y$, equal the weight of the dam, and as this is, of course, to be subtracted from the above, we have finally,

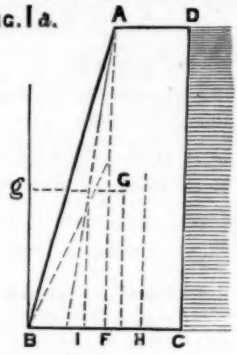
$$h < f \left\{ (2b + a + a') \left(\frac{y'}{y} - 1 \right) - a' \right. \quad 9.$$

These equations are applicable not only to the sliding of the entire dam on its foundation, but also to any particular layer of stone at any point in the dam. The value of the co-efficient of friction f will of course be very different in cases where we consider the stability of different parts of the wall, from that in cases where we consider the dam to slide on an earthty foundation. In the former case, it is that of masonry on masonry, in the latter, that of masonry on earth, and in general clay. In fact, it may be restricted almost solely to clay, because in a sandy, porous or yielding soil, it is better, on principles of economy, not to build a dam, but a dyke. For masonry on masonry, or, indeed, bricks on bricks, we may with safety take the co-efficient of friction as equal to .67; for masonry on dry clay .51; but for masonry on wetted clay the co-efficient falls to .33.

A few examples may, perhaps, serve to illustrate the above remarks. We shall confine ourselves first to the case of rotation about one of the joints, as that is really the most likely one to arise in practice:

Let Fig. 1*a* represent the profile of a

Fig. 1a.



dam, constructed say of brickwork weighing 112 pounds per cubic foot. Let the thickness on top be 10 feet, and that at the base 20 feet, required to find the perpendicular height, the dam must have in order that, when the water stands at the brim, the wall shall be just on the point of turning about the point B under the pressure of the water. Denote by h the height of the dam, or the quantity we are in search of, = C D. Now, by equation 2, the thrust of the water on one lineal foot of surface is $\frac{h^2}{2} \times 62.5$ lbs., and the *moment* of this thrust is $\frac{h^2}{2} \times 62.5$ lbs. $\times \frac{h}{3}$ or $\frac{h^3}{6} \times 62.5$ lbs. The pressure of one foot of the dam, or what is the same thing, its weight is $\frac{\text{A D} + \text{B C}}{2} h \times$ 112 lbs., or $\frac{10+20}{2} h \times 112$ lbs. $= 1680\, h$ lbs., and the *moment* of this pressure with reference to the point B is $1680\, h \times$ B E. Before we can obtain this moment, then, we must find the value of B E, and this is found as follows:

It is evident from a moment's inspection of Fig. 1*a*, that the area of

A B C D × G g = area A B C F × B H + area of A B F × I B, or

denoting A D by a; B C by b; D C by c; and G g by d, we have since $\text{B H} = \frac{2b - a}{2}$, and $\text{I B} = \frac{2(b-a)}{3}$.

$$c\,\frac{b+a}{2}d=ac\times\frac{2b-a}{2}+\frac{c(b-a)}{2}\times 2(b-a)$$

dividing by c

$$\frac{b+a}{2}d=a\times\frac{2b-a}{2}+\frac{(b-a)^2}{3}$$

$$BE=d=\frac{2b}{3}-\frac{a^2}{3(a+b)}$$

Substituting for the above quantities their values, we have:

$$d=\tfrac{40}{3}-\tfrac{100}{90}=\tfrac{110}{9}.$$

The moment of the dam therefore is $1680h\times\frac{110}{9}$.

$$\therefore\ \frac{d^3}{6}\times 62.5\text{ lbs.}=1680h\times\tfrac{110}{9}.$$

$$\frac{62.5h^3}{6}=\frac{184800h}{9}$$

$$h^2=\sqrt{197.12}$$

$$h=44.3982.$$

Again, preserving the same dimensions, let it be required to find the "modulus of stability" of a masonry dam of the profile, shown in Fig. 1, the stone weighing 200 pounds per cubic foot. Draw from the middle of the top A D to the middle of the base B C the line R V, and take its length as 45 feet, and the depth of the water behind the dam, 44 feet. Now, by geometrical principles, which it is not worth while to repeat here, we have:

$$Vg=\tfrac{1}{3}RV\frac{BV+AD}{BV+AR}\text{ or }Vg=\frac{45}{3}\times\frac{10+10}{15}=\frac{245}{15}$$

g being the centre of gravity of the wall. Again, in the two similar triangles R V S and g V T, we have:

$$RV:VS::Vg:VT.$$

The value of V g we have just found. V S is evidently equal to V C − S C, or 10 − 5 = 5. In the triangle R V S, we also have $RS^2=RV^2-VS^2$, or $RS^2=(45)^2-(5)^2$; hence R S = 44.38. Substituting these values in the above proportion, we shall have:

$$45:5::\frac{245}{15}:VT\ \ \therefore\ VT=1\tfrac{2}{3}$$

The weight or pressure of the wall acting through the centre of gravity g of the dam is, as we have already seen, $\frac{20+10}{2}\times 1\times 44.38\times 200=133140$ lbs., and that of the water $44\times 1\times\frac{44}{2}\times 62.5=60500$ lbs. If now we denote by P the "centre of pressure" of the water, that is to say, that point where a single pressure will counterbalance the thrust of the water against the entire face D C of the dam, then $P=C\,P=\frac{44}{3}=14.6$ feet. The quantity we are in search of, the modulus of stability of the wall is the ratio of T B to T O. The value of T B we have already, and may obtain that of T O from the proportion that the pressure of the dam is to the height of the centre of pressure (P) of the water above the base of the dam as the pressure of the water is to the entire pressure of the water acting on its centre of pressure P. Thus:

$$133140:14.6::60500:x$$

$$x=6.6=TV.$$

Dividing this last found quantity by T B, we have:

$$\frac{TV}{TB}=\frac{6.6}{11\frac{2}{3}}=.537=\text{modulus of stability.}$$

In a well built structure, this quantity should never be *less* than .5, hence, as in the present case, the modulus is somewhat above this value, we are justified in regarding the dam as a perfectly stable structure, when the water is not over 44 feet in depth.

In these considerations, we have taken no account of the resistance offered by the adhesion of the mortar. Should this be taken into account—and it is always best that it should not—then equation 9 will require to be modified somewhat as follows: Let H equal the distance of the centre of gravity of a layer of stones below the top of the dam. The shove of the water tending to throw down this portion of the dam is, as we have just seen, $\frac{\delta H^2}{2}$, in which expression δ is merely a short notation for ly. The forces resisting this shove are the friction of the two layers sliding on each other, and the adhesion of the masonry. The first is proportional to the weight of the masonry above the stratum in question, and the second or adhesion of the masonry is proportional to the thickness of the dam at this point. Representing as

before the co-efficient of friction by f, by c the cohesion of the mortar per unit of surface, by s the area of the upper surface of the course next below, and by b the thickness of the dam at this section, we shall have for the resistance R to sliding :

$$R = s\,\delta' f + c\,b$$

and therefore, in order to insure stability, we must have :

$$s\,\delta' f + c\,b > \frac{\delta\,H^2}{2}$$

or clearing of fractions, and then dividing by $\delta\,H^2$,

$$2\frac{(s\,\delta' f + c\,b)}{\delta\,H^2} > 1 \quad . \quad . \quad . \quad 10.$$

Neglecting the adhesive power of the mortar, the above becomes :

$$\frac{2\,s\,\delta' f}{\delta\,H^2} = 1 \quad \text{or} \quad f = \frac{\frac{\delta\,H^2}{2}}{s\,\delta'}$$

The second case of slipping, or that of the dam on its foundation will rarely, if ever arise, when the dam is founded on a rock, for in that case the value of the co-efficient of friction will be the same for the horizontal section of the foundation as for any section of the masonry. It is, however, very likely to arise whenever circumstances will not enable us to lay the foundation on bed rock. In such cases the soil will almost always be of an argillaceous nature, for, should it prove to be of a gravelly, sandy or very permeable character, the employment of some common form of dyke will be much preferable to the construction of a dam. We may, therefore, reasonably assume that in all cases where the foundation course does not rest on a rock surface, it will be laid on argillaceous soil, and as this will readily give, under the action of water, a slippery slimy surface, we must assume a co-efficient of friction very much less than that used for masonry on masonry. With this point kept clearly in view, the conditions of stability will be given by the above equations. Yet there are one or two other considerations that must not be overlooked. Thus, as the stability will depend in large measure on the lateral resistance of the soil, it is not sufficient to be sure that this resistance is large enough to prevent the sliding of the wall, but is also necessary to be assured that at any point of the front of the foundation wall, the normal pressure does not exceed the limit R′ of which the soil or the wall is susceptible. Again, in order to prevent any slipping likely to arise from the lateral compression of the earth, it is not necessary to interpose any packing between the face of the wall and that of the ditch, and, finally, that in all cases it never comes amiss to "step" the rock or the earth on which the foundation course rests, a matter to be considered more in detail hereafter.

SECOND CONDITION OF STABILITY.

To return now to the second condition of stability, namely, that in no point of the structure may the material employed, or the soil of the foundation, be required to bear too great a pressure. For this purpose let A B C D (Fig. 3) repre-

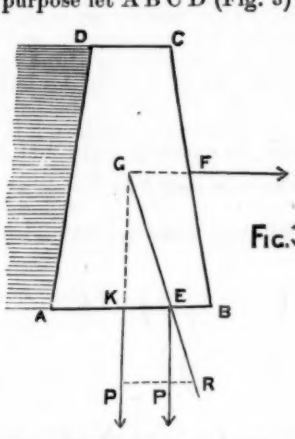


Fig. 3.

sent the profile of a dam. Then from the principles we have already established, it follows that any section of this, equal in length to a lineal unit, may be considered as subject to the action of two forces, which are, respectively, the vertical component P of the *resultant* of the weight of the structure above that unit, and the horizontal pressure or *thrust* of the water, and the horizontal component F of the thrust of the water. In the section A B C D, these two forces act through the centre of gravity G, and produce a resultant of their own which cuts the A B at E. This latter resultant R may therefore be regarded as applied directly to the point E, and resolved into

two components, one vertical and equal to the force P, and one horizontal and equal to the force F. The horizontal force tends to slide the wall along the base A B. This we have considered. The vertical spreads itself over the base from the extremity B, which is nearest the point of application of the resultant, according to the well known decreasing law. Now, in all works on mechanics, we have given a formula which applies to a homogenous rectangle, pressed by a force acting upon one of the symmetrical axis, and this is :

$$p = \frac{N}{\Omega}(1+3n) \quad . \quad . \quad . \quad (\alpha)$$

and

$$p' = \frac{N}{\Omega}\left(\frac{4}{3\,(1-n)}\right) \quad . \quad . \quad (\beta)$$

Where N is the entire load or pressure, and Ω the entire area of the surface pressed. In the case we are considering, the quantity N in equations α and β, is, of course, represented by P the vertical component. Ω, by l, if by this letter we designate the breadth of the base A B, and if we denote the distance E B by u, then will the quantity n in equations α and β be represented by $\frac{l-2u}{l}$.

Substituting these quantities, we shall have :

$$p = \frac{P}{l}\left(1 + \frac{3\,l-6\,u}{l}\right) = \frac{P}{l}\left(\frac{l+3\,l-6\,u}{l}\right)$$

$$= 2\left(2 - \frac{3\,u}{l}\right)\frac{P}{l} \quad . \quad . \quad . \quad . \quad . \quad 11.$$

and

$$p = \frac{P}{l} \times \frac{4}{3\left(1 - \frac{l-2\,u}{l}\right)} = \frac{P}{l} \times \frac{4\,l}{6\,u},$$

$$= \frac{2}{3}\frac{P}{u} \quad . \quad . \quad 12.$$

Equation α is applicable in all cases where $n < \frac{1}{3}$, and therefore equation 11 is applicable when $\frac{l-2\,u}{l} < \frac{1}{3}$; that is when $u > \frac{1}{3}\,l$.

Equation β is applicable to all cases when $n > \frac{1}{3}$, and consequently equation 12 to all cases when $\frac{l-2\,u}{l} > \frac{1}{3}$, or, what is the same thing when $u < \frac{1}{3}\,l$. We have seen that the condition of stability requires that some limit, R′, should be placed on the pressure each superficial unit is expected to bear. The pressure at the point B, must therefore be less or never greater than R′, and we shall have according as u is greater or less than $\frac{1}{3}\,l$,

$$2\left(2 - \frac{3\,u}{l}\right)\frac{P}{l} = \text{or} < R' \quad . \quad 13.$$

and

$$\frac{2}{3}\frac{P}{u} = \text{or} < R' \quad . \quad . \quad . \quad . \quad . \quad 14.$$

And this condition is to be fulfilled for each section made in the profile, neglecting the force of cohesion of the mortar which is unfavorable to resistance.

These expressions are susceptible of yet further modification, if we introduce into the calculation the maximum height λ that may be given to a wall with vertical faces, so that the pressure upon the base shall not exceed the limit R′ of safety. Indeed, if we represent the density of the masonry, or the weight per cubic yard by δ, we shall have $R' = \delta'\lambda$, and the above equation become :

$$2\left(2 - \frac{3\,u}{l}\right)\frac{P}{l} = \text{or} < \delta'\lambda = 2\left(2 - \frac{3\,u}{l}\right)$$

$$\frac{P}{\delta'\,l} = \text{or} < \lambda \quad . \quad . \quad 15.$$

and

$$\frac{2}{3}\frac{P}{u} = \text{or} < \delta'\lambda = \frac{2}{3}\frac{P}{u\,\delta'} = \text{or} < \lambda \quad . \quad . \quad 16.$$

The conditions expressed in these equations would be quite sufficient if the water was always up to the top of the dam, but as this is by no means always the case, the wall must be capable, even when the dam is quite empty, of supporting its own weight without being subject at *any* point to a pressure per unit of surface exceeding the limit $\delta'\lambda$.

In this case the resultant of all the forces acting on the wall is reduced to the weight P′, and denoting by K A, the distance from the resultant passing through the centre of gravity of Fig. (3) to the nearest extremity A of the base, by u, the pressure at A, will be given according to circumstances by equations 11 or 12, and the *stability* of the wall will require that one of the relations expressed in equations 15 or 16 be satisfied when P′ is substituted for P.

The next step, therefore, is to determine the proper

PROFILE FOR A DAM HAVING ONLY ITS OWN WEIGHT TO CARRY.

In order to study under all conditions, the question we are now about to consider, it is perhaps well to inquire, in the first place, what form it is most *convenient* to give a dam having only its own weight to carry, in order that each point of the masonry shall not be subjected to a pressure larger than the limit of safety, and then to determine the alterations which economy require to be made in this *assumed* profile. It is evident, to begin with, that when the height of the dam is such that it does not go over the limit λ (*i. e.* the greatest height we can give to a vertical wall, without the pressure on the base becoming larger than R′, we shall be quite justified in giving the dam vertical facings, and that, in such case, the load for each unit of surface at the lower part will be somewhat less than $\delta'\lambda$, or at least, never greater.

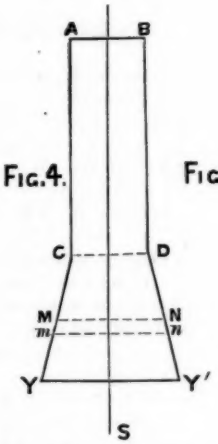


FIG. 4.

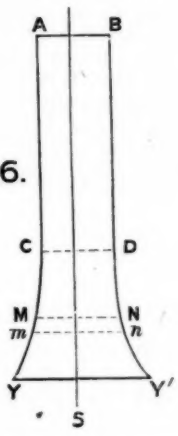


FIG. 6.

unit is nowhere greater than $\delta'\lambda$, and therefore from A to C the face is vertical, but *below* C D, the load exceeds the limit and increasing at each section to the base, and hence from C to Y the face is sloping. And just here we are met by the great question in dam construction that of *profile*. Should the bulging portion C Y Y′D, be bounded by right lines as in Fig. 4, should it be *stepped*, should it be curved, and if so, should the bounding curves be logarithmic curves, simple or compound? these are questions we propose to consider.

Again, we know that whenever the pressure on a horizontal surface of masonry is larger than the limit of safety, we may correct this, by enlarging the area of the surface pressed, and so lessen the load on each superficial unit. And these are the two fundamental principles of dam construction, and may be summed up in brief as follows: If we are constructing a dam of a height equal to or less than λ, *and having only its own weight to support*, it is a safe practice to give it vertical facings from top to bottom. If, however, we are constructing a dam of a height *greater* than λ, *yet having only its own weight to support*, we must make the faces vertical for a distance from the top equal to λ, and *from* this point to the *base* slope them *outward*.

A dam constructed on this latter principle would give a profile similar to that in Fig. 4. From the summit A B to the section C D, the pressure per superficial

It is an easy matter to determine the force to be given to the facing, so that the condition that the load per unit of horizontal surface shall never go over the limit $\delta'\lambda$, shall be satisfied. To do this, we may choose arbitrarily one face and then determine the other, but if we desire to use the minimum of material consistent with perfect safety, then the wall must be symmetrical as to its axis. In such a case as that illustrated in Fig. 4—that of a high masonry dam, whose height is greater than λ—the slopes D N Y′ and C M Y, ought to satisfy the

requirement that, if in any section, as M N, the load per surface unit is equal to any given quantity, the pressure will be the same for any other section as $m'n'$, infinitely near to it. This will be fulfilled, if the increase given to the base is proportional to the increase of pressure, or as the profile is to be made symmetrical to the axis O S, if the increase of the half surface L N or L M is proportional to the increase of load on that half surface. If we denote by P the pressure on L N, arising from the weight of the structure above, and a the surface of this section, then, it is evident, the above condition will be expressed by

$$dP = K.\,da. \quad . \quad . \quad . \quad 17.$$

In which K is a constant quantity, and denotes the limit of pressure on the unit of surface or $\delta'\lambda$. Again, by b, denote the dimensions of the dam in the direction perpendicular to the section we are concerned with, and by x the length of the half section L N, or, to express it mathematically, the abscissa of the curve or line sought (*i. e.* D N Y'), and finally, by y, the distance of M N from a horizontal line taken as the axis of x. Then the surface a will equal to bx, and consequently an increase of surface as da in equation 17, will be expressed by

$$da = d\,b\,x$$

and moreover

$$dP = \delta'\,b\,x\,dy$$

These values substituted in equation 17 give for the differential equation of the curve,

$$\delta'\,b\,x.\,dy = K.\,b.\,dx \quad . \quad . \quad 18.$$

whence

$$dy = \frac{K}{\delta'}\,\frac{dx}{x}$$

But K equals the limit of pressure per unit or $\delta'\lambda$, and this value replaced for K, we shall have

$$dy = \frac{\delta'\lambda}{\delta'} \cdot \frac{dx}{x} \text{ or } dy = \lambda\,\frac{dx}{x}$$

Integrating this between the proper limits, we shall have

$$y - y_0 = \lambda \log\left(\frac{x}{x_0}\right) \quad . \quad . \quad . \quad 19.$$

Now, from this equation we see that, the curve being referred to rectangular axes, one of the co-ordinates is equal to the logarithm of the other, and, hence, the curve must be a *logarithmic curve.* Here then we have one property of the curve D N Y. To find in the next place the origin of its co-ordinates, we may make in the foregoing equations $x_0 = \lambda$, in which case we shall have:

$$1 = \frac{dy_0}{dx_0} \quad \text{and} \quad y_0 = 0 \quad . \quad 20.$$

From this last relation it is quite apparent that the origin of co-ordinates is to be taken at a point where the value of x is equal to that of λ, and in this point the tangent to the curve makes an angle of 45° with the axis of x. Returning now to equation 19, let us replace y_0 and x_0 by their respective values, given in equation 20, when we shall have:

$$y = \lambda \log.\,\frac{x}{\lambda}$$

or passing from the system of Napier to the common system of logarithms,

$$y = 2.302658509\ \lambda \log.\,\frac{x}{\lambda} \quad . \quad . \quad 21.$$

This curve, when constructed, will give the form of the facing of a wall of indefinite height for which the pressure per unit of surface equals the limit of pressure K. It is not to be forgotten in in making use of equation 21, that the direction in which y's are usually estimated has been reversed; in other words, y when positive is to be estimated downwards, and when negative upwards, or in the direction of L O. Fig. 5 represents this curve constructed, by

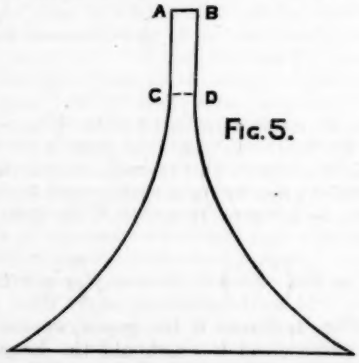


Fig. 5.

* We may also pass from the Naperian to the common system, by multiplying the Naperian logarithm by the modulus of the common system, which is 0.434294. Its logarithm is 9.637784.

assuming the pressure limit or K as 132,000 lbs., and the density of the masonry as double that of water.

In such a profile, as Fig. 4 has, the sloping faces below C D being bounded by right lines, we may obtain the necessary breadth of the base Y Y′, as soon as we have determined the height and the breadth at top. Denote by b the breadth at top A B; by h the distance A C$=\lambda$, and by h' the distance from C to the base Y Y′; by δ' the density of the masonry, and by x the quantity we are seeking for, or the base YY′. Then we shall have:

$$\left\{h\times b+h'\left(\frac{b+x}{2}\right)\right\}\frac{\delta'}{x}=\delta'\times h \quad . \quad 22.$$

The quantity h in this equation, which is merely another expression for the quantity λ, has been determined by a number of investigators, but the most reliable results are those obtained by the French engineers,* who, in the construction of their great masonry dams, such as Furens, have taken the limit of pressure K at 60,000 kilogrammes, or about 132,000 lbs. per square metre, and K being equal to $\delta'\lambda$, and δ' being equal to 2,000 kilogrommes, λ becomes equal to 30 metres. As we shall hereafter see, however, the limit of pressure varies for the outer and inner face of the dam.

If, again, the profile adopted be such as is illustrated in Fig. 3, that is to say, if the faces of the dam slope continuously from the top to the bottom, then the thickness or breadth of the base will evidently be obtained by dividing the product of the height of the wall and its thickness on top by the difference between 2 λ and the height. For $\delta'\lambda$ or the limit of pressure is equal to the area of the profile, multiplied by the density of the masonry divided by the thickness of the base. In the figure, the area is plainly equal to half the sum of the two parallel sides by the altitude, and denoting this latter by H, we shall, therefore, have:

$$\delta'\lambda=\mathrm{H}\left(\frac{b+x}{2}\right)\frac{\delta'}{x} \quad \text{or} \quad x=\frac{\mathrm{H}\,b}{2\,\lambda-\mathrm{H}} \quad . \quad 23.$$

The conditions which govern the construction of such a dam, and the height to which it is safe to build it, become from this equation quite apparent, should we make H$=2\,\lambda$, then x would equal $\frac{\mathrm{H}\,b}{0}$, and the base of the wall would spread out to infinity. Should we, upon the other hand, make H greater than 2 λ, then λ would become negative, and hence it follows that the greatest height we can give to a masonry dam with straight sides equally inclined from the summit and not go over the limit of resistance for masonry, is equal to twice that of a wall with vertical sides. Yet, within this limit, such a profile for a masonry dam of any height, occasions a gross waste of material. This becomes strikingly apparent, if we compare the breadth of base of a dam constructed with inclined faces from top to bottom, with that of a dam of the same height, but having a profile such as that of Fig. 4. Suppose each dam to be 30 metres high and 5 metres thick on top; required the thickness at the base. For the first case, using equation 23, we have:

$$x=\frac{30\times 5}{60-30}=5 \text{ metres.}$$

For the second form of profile, we use equation 22, and have, since the quantity h equals λ, the same value, or $x=5$ metres.

If we raise the dam by 10 metres, then equation 23

$$x=\frac{40\times 5}{60-40}=10 \text{ metres.}$$

and by equation 23

$$\left\{30\times 5+10\left(\frac{5+x}{2}\right)\right\}\frac{2{,}000}{x}=60{,}000$$

or since

$$x=\frac{b\,\delta'\,(2\,h+h')}{\delta'\,(2\,h-h')}=x=7 \text{ metres.}$$

If, once more, we add ten metres to the height, then equation 23

$$x=25 \text{ metres.}$$

and eq. 22 $\quad x=10$ metres.

The saving thus affected when the dams are of great height becomes simply enormous. The difference, however, between the profile when the dam below C D (Fig. 4) is bounded by right lines, and when bounded by logarithmic curves, such as shown in Fig. 6, is not so marked as in the cases just considered, yet is

* MM. Delocre, Sazilly and De Graeff.

considerable. To take but one case in illustration, a dam of a profile such as Fig. 6 illustrates, with the faces below C D bounded by curves, would require (equation 21) a breadth of base equal to 9.739 metres, the height and thickness at top being as before, 50 and 5 metres respectively, while, as we have just seen, if the faces below C D were right lines, the base would be 10 metres.

Such, in brief, is the relative merit of these three forms of profile, for a dam having nearly its own weight to support. In practice, however, such a dam can, of course, never exist, and it thus becomes necessary to take into consideration the second condition, or that of a dam supporting a charge of water.

PROFILE FOR A DAM RESISTING THE PRESSURE OF WATER.

And here, again, we are to throw aside, at first, all practical considerations, and determine a theoretical profile of equal resistance, one in every part of which the pressure shall not be greater than the limit R′. For this purpose we return to the two equations, deduced some time back, which express the conditions of stability for a dam resisting the thrust of water, and neglecting the signs > and < and the values corresponding to them, take only those corresponding to the sign of =. We then have the two following equations :

$$2\left(2-\frac{3u}{l}\right)\frac{P}{\delta' l}=\lambda \quad . \quad . \quad 24.$$

and

$$\frac{2}{3}\frac{P}{u\delta'}=\lambda \quad . \quad . \quad . \quad . \quad . \quad . \quad 25.$$

If we now replace the quantities u, l and P, by their respective values, expressed in functions of the height of the dam, we may readily deduce two equations which, on examination, will show two things.

1°. That the profile offering the least thickness, consistent with the conditions of stability, is one in which the side turned towards the water, has a vertical face, and the side turned from the water, or the outer face of the wall, a concave face.

2°. That as the height increases, the thickness increases less rapidly, so that in a wall constructed with a vertical face on the water side and a curved face on the other side, and so planned that it shall satisfy the conditions of stability as to its base will present an excess of strength for the surplus of height.

Fig. 7 is the profile of a dam of this

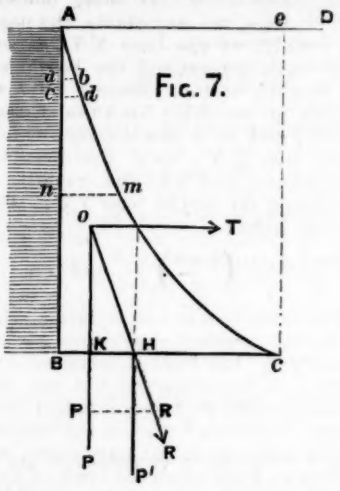


FIG. 7.

description. It will be observed, moreover, that in this form of profile the thickness of the wall at the top is zero. This, of course, in practice is never admissable, inasmuch as it presupposes the water to be at all times in a perfectly quiescent state, and thus makes no allowance for the very considerable force of the waves raised by the wind. It is, therefore, necessary, whatever the profile, to give the dam quite a thickness at the summit, in general, about fifteen feet, is a good width, as it thus enables us to construct a footpath and roadway on the top of the dam, which is quite a convenience.

Before we consider any other modifications, it may be well to determine as nearly as possible the co-ordinates of the concave curve forming the outer face. For this purpose we will take the vertical face A B as the axis of x, and for the axis of y, a perpendicular to this passing through the point A, and call it AD. Anywhere on the curve we will take a point C, and denote its co-ordinates B C $=y$ and C $e=x$; then the relation existing between x and y will give the equation of the curve. Now, as we have already seen, the wall is subject to the action of two forces, the weight of the dam P, which acts vertically downwards

through the centre of gravity and the horizontal thrust T of the water. These two forces produce a resultant R, which cuts the base of the dam in this case at the point H. This resultant, therefore, may be regarded as applied directly to the point H and resolved into two components, H P and H C, respectively, parallel to O P and O T. We have also seen by equation 5 that the horizontal thrust of the water is equal to

$$F' h y = \tfrac{1}{2} h^2 l y \quad . \quad . \quad . \quad 26.$$

Or replacing h by its value, and ly by its value δ, then T, or the horizontal thrust of the water, equals

$$T = \frac{\delta x^2}{2} \quad . \quad . \quad . \quad . \quad 27.$$

And in the same way

$$P = \delta' \int_0^y y \, dx \quad . \quad . \quad . \quad 28.$$

Returning now to equations 24 and 25, we find that the quantity l is equal to y, and that we have therefore to determine the value of u in functions of x and of y. Now u equals H C and H C = K C − K H. The triangles O P R and O K H, moreover, being equiangular triangles are similar, and have their like sides proportional, and

$$K H : P R :: O K : O P$$

or

$$\frac{K H}{P R} = \frac{O K}{O P}$$

or to express the equality in terms of T, x and P,

$$\frac{K H}{T} = \frac{x}{3 P} \quad . \quad . \quad . \quad . \quad . \quad 29.$$

Replacing in the 29th equation the values of T and P, as obtained in the 27th and 28th equations, we have:

$$\frac{\frac{K H}{\delta x^2}}{2} = \frac{x}{3 \delta' \int_0^y y \, dx} = K H = \frac{x}{\frac{3 \delta' \int_0^y y dx}{\frac{\delta x^2}{2}}}$$

$$= \frac{\delta x^3}{6 \delta' \int_0^y y \, dx}$$

Or, for brevity, representing $\frac{\delta}{\delta'}$ by D,

$$K H = \frac{D x^3}{6 \int_0^y y \, dx} \quad . \quad . \quad . \quad 30.$$

This gives us the value of K H in the expression

$$u = K C - K H \quad . \quad . \quad . \quad . \quad 31.$$

But K C is evidently equal to $y - B K$, in which B K is the distance from the centre of gravity of the surface A B C to the vertical axis of x or A B. This distance is equal to the sum of the moments of the areas such as $a b c d$, or

$$B K \int_0^y y \, dx = \frac{\int_0^y y^2 \, dx}{2} \quad \text{or again,}$$

$$B K = \frac{\int_0^y y^2 \, dx}{2 \int_0^y y \, dx}$$

$$\text{Hence } K C = y - B K = y - \frac{\int_0^y y^2 \, dx}{2 \int_0^y y \, dx}$$

$$= \frac{2 y \int_0^y y \, dx - \int_0^y y^2 \, dx}{2 \int_0^y y \, dx} \quad . \quad . \quad . \quad 32.$$

Substituting in equations 31, the values of K H and K C obtained in equations 30 and 32, we have:

$$u = \frac{2 y \int_0^y y \, dx - \int_0^y y^2 \, dx}{2 \int_0^y y \, dx} - \frac{D x^3}{6 \int_0^y y \, dx}$$

Or, reducing to a common denominator, and subtracting,

$$u = \frac{6 y \int_0^y y \, dx - 3 \int_0^y y^2 \, dx - D x^3}{6 \int_0^y y \, dx} \quad . \quad 33.$$

Thus, then, we have the value of u in functions of x and y, and substituting this value for u in equation 24, and remembering that $l = y$, we have:

$$\frac{4\,y\,P-6\,u\,P}{\delta' y^3}=\lambda$$

$$\frac{\begin{array}{l}4\,\delta' y\int_o^y y\,dx-36\,y\,\delta'\int y^3\,dx\\ -18\,\delta'\int_o^y y^3\,dx-6\,Dx^3\,\delta'\int_o^y y\,dx\end{array}}{\delta' y^3\int_o^y y\,dx}=\lambda$$

$$\frac{\begin{array}{l}24\,\delta' y\int_o^y y^3\,dx-36\,y\,\delta'\int_o^y y^2\,dx\\ +18\,\delta'\int_o^y y^3\,dx+6\,Dx^3\,\delta'\int_o^y y\,dx\end{array}}{6\,\delta' y^2\int_o^y y\,dx}=\lambda$$

$$\begin{array}{l}-12\,y\,\delta'\int_o^y y^3\,dx+18\,\delta'\int_o^y y^3\,dx\\ +6\,Dx^3\,\delta'\int y\,dx=6\,\delta'\lambda y^2\int_o^y y\,dx\end{array}$$

Dividing both members of the last equation through by $6\,\delta' y\int_o^y y\,dx$, we shall have, after bringing all terms containing y into the first member,

$$-2\,y\int_o^y y\,dx+3\int_o^y y\,dx+Dx^3-\lambda y^2=0 \qquad 34.$$

By making the proper substitutions in equation 25,

$$3\,\delta'\left\{\frac{2\,\delta'\int_o^y y\,dx}{\dfrac{6\,y\int_o^y y\,dx-3\int_o^y y^2dx-Dx^3}{6\int_o^y y\,dx}}\right\}=\lambda$$

$$2\,\delta'\int_o^y y\,dx=$$

$$\frac{18\,\delta'\lambda y\int_o^y y\,dx-9\delta'\lambda\int_o^y y^2\,dx-3\,\delta'\lambda\,Dx^3}{6\int_o^y y\,dx}$$

$$12\,\delta'\left(\int_o^y y^2\,dx\right)^2=$$

$$18\,\delta'\lambda y\int_o^y y\,dx-9\,\delta'\lambda\int_o^y y^2\,dx-3\,\delta'\lambda\,Dx^3$$

Transposing, after dividing each member by $3\,\delta'\lambda$, we have:

$$0=4\left(\int_o^y y^2\,dx\right)^2-6\,\lambda y\int_o^y y\,dx+3\,\lambda\int_o^y y^2\,dx+\lambda\,Dx^3 \quad . \quad . \quad . \quad 35.$$

But here a new difficulty presents itself, for no sooner do we attempt to integrate equations 34 and 35, than we see it is quite impossible to perform the integration by any exact method. We may, however, obtain an approximately correct solution by finding the value of y in a series of functions x. Treating equation 34 by this method we obtain, says M. Delocre, for y the value

$$y=a\,x^{\frac{1}{2}}+b\,x^{\frac{2}{3}}+c\,x^{\frac{3}{4}}+d\,x^{\frac{4}{5}}+e\,x^{\frac{11}{12}}+f\,x^{\frac{12}{13}}+\&c. \quad 36.$$

While equation 35 gives:

$$y=a\,x+b\,x^2+c\,x^3+d\,x^4+e\,x^5+f\,x^6+\&c. \qquad 37.$$

These equations, as it is quite apparent, are of no earthly value for practical purposes, and we shall, therefore, drop all further consideration of them. Indeed, if it were possible to obtain the equations of the curve A m C, by a short and simple process of integration, a moment's reflection will show that such a profile as that illustrated in Fig. 7 would not be suitable for practical use. For this profile has been calculated on the hypothesis that the dam is *always* to support a head of water equal to its height, and in this case the pressure on any horizontal section as $m\,n$ will, it is quite true, not exceed the limit R. But as it happens that the dam is very likely to be at times empty, the profile must be such that, full or empty, the pressure on any section as $m\,n$ shall not be greater than R. We know that this limit will not be exceeded for the face of the wall bounded by A m C, and it thus remains to consider only the vertical face A B. On reference to the calculations we have made relative to the profile of walls having only their own weight to support, it becomes noticeable that the limit will soon be passed if the wall is slightly raised. Supposing this limit to be reached at the point n, we are forced for the sake of stability to depart from the vertical below this point, to give the water face a swelling or bulging surface, and thus adopt a profile similar to that illustrated in Fig. 8. This profile is supposed to fulfill the conditions that, at any section

as $d\,e$, taken below $m\,n$, the pressure at the point e, the dam being full, will be less than or equal to the limit R, and the dam being empty, the pressure at d resulting from the weight of the structure, will also be less than or equal to the same limit of pressure, R.

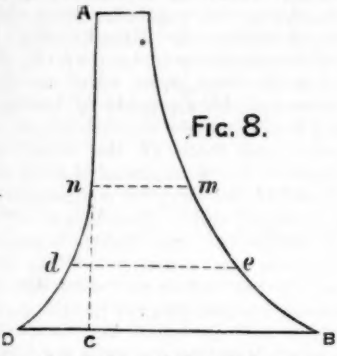


Fig. 8.

This last modification, moreover, is one of no small importance, as it enables us to correct some of the chief errors in which the theoretical consideration has unavoidably led us, and thus to approach nearer to the end in view; the determination of a profile of equal resistance suitable to practical requirements. If the two curves $m\,e$ B and $n\,d$ D could be readily obtained by the above formula, the profile of Fig. 8 would answer almost all necessary conditions as to stability and economy; but they cannot. It therefore remains to do the next best thing, and to replace the curved surfaces, by polygonal surfaces of as small sides as possible—in order that they may approach reasonably near to the curves—and then determine the equations of these sides of the polygons; or to adopt a similar method to find the equations of the two curves in question. This we shall now endeavor to do. It is, however, to be remarked that there are two notable instances of the use of the form of profile, shown in Fig. 8; that of the dam at Furens, and that constructed on the Ban, a tributary of the Gier, by M. Mongolfier. Each of these we shall consider later.

OCEAN STEAMSHIP NAVIGATION—ITS PERILS, AND HOW TO AVERT THEM.

By Capt. S. P. Griffin.

Written for Van Nostrand's Magazine.

I.

Seamen believe that the perils of ocean navigation can be very much lessened by correcting some of the evils which are well known to them. Fatal disasters to large passenger steamships are, in many instances, attributed to causes well understood beforehand, and known to have their origin in the continuance of a certain custom on board vessels, and in the unfitness of certain aids to navigation. From amongst the questions relating to the sea that demand attention, I will select two for discussion in this paper; the first one is the danger arising from "Watch and Watch."

From the earliest times to the present, in the internal economy of vessels, both large and small of all nations, in every sea, it has been, and still is the custom, to divide the crew into two watches, working what we designate "watch and watch," that is to say, four hours on and four hours off duty alternately. This plan of work in the days gone by, when vessels were comparatively few and small, and when time was of less value than it is now, apparently did not obstruct, or interfere with the prosperty or the growth of commerce, or the successful navigation of the ocean, but, however well it has subserved a useful purpose in the past, it certainly requires to be changed so far as it is applicable in the present to the mates of ocean going steamships. The exigencies arising from the immense fleets, at all times, in every sea, sailing and steaming in every direction, upon all points of the compass, and

the wonderful increase in the size and speed of first-class passenger steamships render it absolutely neccessary that, with a proper regard for life and property, the deck at all times must be in charge of a man of well ascertained mental and physical abilities. Yet, it is a fact very well known to the initiated, that the custom of "watch and watch," will at times so impair a man's faculties, when thorough efficiency may be most urgently required, as to render him wholly unfit to discharge his important duties; within the experience, and subject to the observation of every one who goes to sea, is the significant reality that a man can not day after day, night after night, in good weather and in bad weather, preserve himself in a condition that qualifies him for every emergency, that inevitably sooner or later, at a critical time, he will be found wanting, and the most serious consequences may become involved in his temporary deficiency, even unto the total loss of the steamship with all on board; hence it is that those ashore hear of the most unaccountable shipwrecks; hence it is that sweet water critics can see the error of judgment, can detect the mistake in navigation, which was the immediate cause of an appalling disaster.

I will describe the duties of a mate at sea, a mate who is intrusted with a watch, he who has charge of the deck of a steamship, freighted with two thousand tons of cargo and a thousand human lives; the steamship runs at a speed of fourteen knots an hour across a crowded sea, upon a dangerous coast, in sunshine or in darkness, in rain or in fog; it is all the same, on she goes with life or death awaiting her, depending to a considerable extent upon the skill, courage and self-possession of this man. His station is upon the forward bridge, within easy call to the man at the wheel, close to the standard compass, and to an engine bell pull. He must not on any account leave it, unless he is regularly relieved. He must keep on his feet, in constant motion, to see that the ship is on her assigned course, that proper sail is carried, that the yards are trimmed, that order is maintained about decks, that the rules concerning lights and fires are obeyed; he must keep a bright lookout ahead and all around, he must listen for unusual sounds, he must be ready to detect unusual smells, he must be unceasing in his vigilance during the period of his watch. After he is relieved at the termination of his watch, he must write in the deck-log the remarks that he thinks are necessary to keep up the narrative of the voyage, he must see that the proper entries are made of barometric and thermometric indications, of the direction and force of the wind, the character of the weather, of the sea and the speed of the ship, he communicates with his relief, and he goes below. This is an outline of the duties, when off watch, or in his "watch below," as it is called; he has to take and work out observations for latitude, for longitude, for variation and deviation, and every day soon after Meridian, he must send in to the captain a carefully prepared report of his "day's work," he must attend quarters for fire and boat exercises, he must be always ready to respond to any sudden call for all hands, he must by his example and his teaching help to inspire confidence, and to maintain discipline.

Let us now enter into a brief examination of the effect this custom of "watch and watch," in its application to mates of steamships. Let us endeavor to understand its operation for good or for evil, as it now prevails upon hundreds of them on duty in every sea. Let us learn if there are not dangers of the deep that are probably never thought of, or even known to exist by those who own the steamships, or by the thousands of passengers who go from port to port in them.

Of the twenty-four hours that compose a day, the mate who stands his "watch and watch," spends twelve of them upon the bridge in charge of the deck. In being called ten minutes before the termination of a watch, so as to be ready to relieve when the bell strikes, and in occupations after it one and a half hours are consumed; at his meals, smoking, and another thing or two, three hours; in observations, day's work, reports, exercises, and what not, one and a half hours more, making in all eighteen hours, thus leaving him only six hours for sleep, to be picked up at intervals between the watches, as best he can, subject to innumerable disturbing influences. Is this amount of rest, and the way of getting

it, enough for a working man anywhere? Is it enough for a man who is exposed to the severe trials of a hard winter in our wild North Atlantic? Do we not see from this that it is beyond the power of human endurance to keep up under it? May we not here begin to find an explanation for some occurrences which in their mysteries have hitherto baffled investigation? I assure you that the custom of "watch and watch," the custom that compels a man to undertake more than he is capable of doing, is the ultimate cause of many of the heart rending disasters which have come to us from the sea.

I will present an every-day case to you, it will be at once recognized by any person who is familiar with the "way things are done aboard ship." It may be in a prolonged gale of wind, in winter, cold, black and gloomy, spray flying everywhere, and freezing where it falls, the decks are slipping with ice, and dangerous from the violent motion of the ship; or it may rain incessantly, or it may be an impenetrable fog. The mate who takes charge of the deck at Meridian, is relieved at 4 P. M., and soon afterwards he goes below; he gets his supper, enjoys his smoke, and at 6 P. M. again takes charge of the deck, for the second dog-watch, which terminates at 8 P. M. As soon as he is relieved he writes up the deck-log, then hurries below, shakes off his dunnage, and by one bell—half past eight—he is turned in and asleep. At ten minutes before midnight he is called for the mid-watch, from 12 to 4 A. M. He has had less than three and a half hours sleep; he went below in wet clothes, he comes on deck in wet clothes, aching and weary from past exertions and insufficient rest. He takes charge, he receives the course, the orders, a statement of the condition of things; he inspects the compass, speaks to the man at the wheel, hails the look-out, cautions the "watch" to stand-by, he shakes himself for warmth, and commences to walk the bridge; he exerts himself to be faithful to his trust, to discharge his responsible duties to the best of his abilities, but, before the termination of his four hours, the time begins to drag wearily along, and at last he becomes conscious of his weakening faculties, aware of the danger that will attend his neglect, and of the accountability to which he will be held, he makes a struggle with himself, and barely succeeds in keeping himself awake; those who keep "watch and watch" in charge of the decks of steamships know all of this well enough; those who have not had the experience, and who doubt the accuracy of the statement, can easily find proof of it, if, in the next passage they are going, they will faithfully stand the watches.

Thus far I have spoken of the mate who has "eight hours in," who goes below after 4 A. M. and turns out again before seven bells—half past seven. Then how much more trying will be the case of the mate who has "eight hours out," he who keeps the watch from 8 P. M. to midnight, and again the watch from 4 A. M. to 8 A. M. Is it not utterly impossible for him to look to windward in a north-east snow storm eight hours of a night? Is it not beyond human endurance to remain in a freezing atmosphere, exposed to the fury of wind and sleet, for that length of time, holding on like grim death against the heavy laboring of the ship and not impair the powers of mind and body, the whole strength of which may be on the instant necessary for the safety of the ship? Most persons who travel by sea, if they trouble themselves to think at all about such things, believe that men are able to do it, that they get hardened to that sort of thing, you know, until at last they do not care the least bit about it. The timid, as they lie stowed away in soft blankets in the warm bunks of the after cabin, would shudder at the frequent narrow risks they run if they did but now of them; they would be overcome with horror at the thought that in the dark and fearful night a weary worn-out mate is straining his imperfect vision to make the dim outline of the rock bound shore, upon which the ship is madly rushing; and he cannot see it, nor can he see the approaching sail, nor the warning rays of a light, nor hear the indistinct roar of breakers, nor the feeble tones of a bell, nor catch the presence of unusual elements on-board nearly so well as when he is in good condition.

A mate does not complain about the dangerous effects produced upon him by the custom of "watch and watch"; he

does not confess his absolute physical inability to thoroughly fulfill all of the requirements of his position, for the alternative as the economy of the ship is managed, and of which he has the greatest dread, is to discharge him with a black mark against his name, and to ship another in his place who will not growl and grumble at his ordinary work. Therefore he keeps his troubles to himself, the evils continue unabated, and it happens at last, that a steamship with her freight of life and riches runs swiftly on to meet a terrible fate without a single timely effort having been made to preserve her.

Hence it may be safely declared that risks of collision, of stranding, of fire, in short all risks pertaining to the sea will be very much lessened if the decks of vessels are always left in charge of intelligent men refreshed by sufficient sleep in comfortable quarters, instead of others completely exhausted by excess of work and prolonged exposure. I conscientiously believe that many of the disasters to ocean going vessels are due to the dangers that I have tried to explain, and to others well known to seamen and such as are within the control of man's capacity as a reformer.

Another word or two upon the dangers arising from the custom of "watch and watch" at sea. It is not only in dark or foggy weather that accidents occur, it is not only in long continued winter gales that ships are lost; but with the moon and stars shining out in all of their glory, in a beautifully transparent atmosphere, in warm tropical nights, in unruffled water, in a dead calm, vessels run into each other, and others run squarely ashore. The drowsy mate could not see, or seeing could not comprehend, or did not act in time to avert an impending calamity. Instances of this kind are by no means uncommon, and I could easily relate a number of them perfectly well authenticated, but in doing so I might make invidious distinctions, and direct censure where as we have seen there may be mitigating circumstances in the case. But proofs may be found in the marine columns of the daily newspapers by those who know what meaning to put upon the reports.

The reform that is necessary to correct the evils spoken of—the change in a long established custom of the sea, that will oftentimes save ships from destruction, is not in itself a very great one, it is inexpensive, and it can be made at once without any derangement of good order and discipline—it is this: put mates of ocean going steamships in three watches, instead of keeping them in two, give them four hours on and eight hours off watch.

As steamships are manned now, this change can be made without any increase to the compliment allowed to them, therefore there will not be any expense attending it.

Let us inquire into the operation of the three watches rule, and see how it affects the mates. He who takes the watch from M. to 4 P. M. has time for his supper, a smoke and a snooze, before he is called for the first watch. At 8 P. M. he is again on the bridge, where he remains until midnight, he then goes below until 8 A. M., then, once more, he takes charge for the forenoon watch, under this rule he has eight hours in every night, he has time to take proper care of himself, to dry his clothes, to keep his room in order, to be accurate in his day's work, and, far above all else, he is strong in body, clear headed, and self-possessed when he takes charge of the deck—he fully and faithfully performs the duties as officer of the watch.

The second topic that I desire to discuss, in this paper, is the insufficiency of our present sea coast fog-signals as aids to navigation.

When fogs prevail along our coasts, there are in use as a means of warning the adventurous mariner of his proximity to danger, three kinds of instruments for producing sound signals. The first in the order of usefulness is the siren, the next the whistle, and then the bell. The approach to harbors, light-boats and light-houses, on outlying rocks are furnished with one or the other of these. The theory with regard to their use is, that they are capable of emitting sounds of such intensity as will be conveyed to a distance from a given danger, sufficiently great for a vessel to perform any evolution in, that may be at the time essential to her preservation.

Now the important question to be answered as preliminary to the selection of an instrument for producing sound

signals during fogs or in thick rainy weather is this; what is the least distance to which a vessel may approach a danger and still have reasonable space enough to enable her to extricate herself from it or to avoid it?

A steamship of about four hundred feet in length, that steers well, running at full speed, with the helm hard over, will go around in a circle the diameter of which is about five thousand four hundred feet, or, say nine tenths of a sea mile, in smooth water, without wind in twelve minutes. Let us assume that the swinging of the head, or its motion in azimuth is at a uniform rate, then it will take six minutes in time to head a course directly opposite to the one she was steering, and she will be nine tenths of a mile from the position that she was in when the helm was put hard over, then also it will take three minutes in time to head a course at right angles to the one she was steering, and she will be forty-five hundredths of a mile more in advance of her first position and the same distance to the right or left of it, accordingly as she used the starboard or port helm. In other words under the most favorable circumstances of wind and sea, it will take six minutes to turn and head square away from danger, and the danger must be at least a half mile from where she was when the helm was put over. Under the same favorable circumstances the engines can be stopped and worked back until she gathers steamway in five minutes time and within a distance of a half mile. But the fog signal must be adequate to the case when the steamship is in the most unfavorable circumstances of wind, weather and sea.

Suppose the steamship is running head on to danger, not being aware of its neighborhood, in a rough sea, a strong breeze blowing, all sail set, and a dense fog prevailing, what length of time and what space must she then have to work in? She runs fifteen hundred feet a minute, the sea, the wind and the sails will impede her rapid obedience to the helm, it is known that engines often hang a long time, for a situation like this, before there can be worked astern. Hence we can readily understand that one mile of distance at least is necessary for her safety.

But we must make other allowances, we must adopt a signal that will meet all objections and overcome them. The officer of the watch may not be in a thoroughly wide awake condition, his sense of hearing may be at the moment slightly impaired, he may be slow in reaching conclusions, he certainly will possess some imperfection, and in the peculiar circumstances of the case it may be a perilous one to the ship. For example, his attention is first attracted by a faint and unusual sound that comes to him despite the noises aboard and the swash of the water, it seems like a signal, not knowing that he is near a danger, he listens for a repetition of it, he orders the quarter-master and the look-out also to listen for it, precious time is flying, the steamship is rushing on, when, there it is again, all hear it now—there is no longer any doubt about it, it is right ahead, there is not any time to think; courage, self-possession, and that spontaneous knowledge of what to do on the instant in extreme peril are necessary, he gives at once the proper order to the man at the helm, who may not comprehend it, or may be startled by its peremptory tone, and puts the wheel the wrong way (this is not by any means an uncommon occurrence), the steamer may be sluggish in her movements, the engineer of the watch may misunderstand the bells, or may not be close to the gear, other circumstances may arise of small importance in themselves, but of vital consequence to the welfare of the ship. Therefore, for safety to a long full powered steamship, running under all sail in a dense fog, head on to a danger, she must receive a warning signal at least two miles from it in distance, and eight minutes in time.

In addition to the Mayor of Sheffield's gifts of a park and almshouses to the town, it was announced at a meeting that Mr. Firth had promised to provide, at a probable cost of some £15,000, a building for the lectures and classes commenced there and elsewhere by the universities. These lectures and classes were well attended by members of the working people and others, and it is understood that Mr. Firth will also subscribe £1,000 towards a scholarship fund if £9,000 are raised by the town for this purpose.

ROADS, STREETS AND PAVEMENTS.*

THE announcement of a new work bearing the above title reminds us that standard works well suited to the wants of the present time are not abundant. Especially in the matter of pavements is there sore need of information based on intelligent observation. The numerous failures in city street pavements, within the last few years, have certainly furnished an amount of experience sufficient to be of service as a guide for the future; but to draw a serviceable lesson from such experience requires something more than cursory observation. Only a practiced engineer can properly sum up such results, and draw safe conclusions. Failures in this department of engineering result from a variety of causes; bad material, bad workmanship; an exceptional character in the soil—either of these, may insure such swift destruction, that a verdict of condemnation by an injured community will be so unsparing as to work harm; an excellent material has been often heartily condemned by universal consent, because of a failure that existed in something else.

The name of the author of the present work is a sufficient guaranty that the lessons drawn from recent experiences are well presented, and that the deductions are safe ones.

The treatise is very full upon the subjects of Location and Grades of Country Roads—Earthwork and Transverse Form of Country Roads—Road Coverings—Maintenance and Repairs of Roads—Streets and Street Pavements.

This latter subject is discussed in the fullest manner, and is made to include descriptions of methods of construction of all the pavements that have at any time found favor with the profession or the public.

A chapter on "Hygienic Considerations" is particularly instructive, as the following abstract will prove:

A practical and general recognition of the fact—so well known in the medical profession, and indeed among all ranks of cultured people—that the pavements of a city exert a direct and powerful influence upon the health of its inhabitants, has never been secured. Most people claim simply that a street surface should be durable, smooth without being slippery, and, as a measure of economy, that it shall be durable and easily cleansed; but they go no further.

The advantages of noiselessness are recognized by many upon various grounds; by the large majority as simply conducive to comfort, but by few as conducive to health; while the kind of material used, provided it satisfies the foregoing conditions, and the character of the surface is satisfactory with regard to continuity and impermeability, is far too generally considered to be a matter of small moment.

The hygienic objections to granite, are first its constant noise and din, and second its open joints which collect and retain the surface liquids, and throw off noxious vapors and filthy dust.

In populous towns there is scarcely a moment of silence, night or day. M. Fonssagrives, Professor of Hygiene at Montpelier, says, "I cannot consider such a perpetual vibration of the nerves as harmless even for those who have been born and bred in the midst of the noise. It is certain that it is a very genuine cause of erethisme, and to it must be ascribed the prevalence of nervous temperaments and disease in the large towns. . . I have known a young girl of seventeen years old, suddenly transported from the provinces to a noisy quarter of Paris, show the most alarming symptoms of nervous disorder, which did not subside until she returned to a quieter and less exciting atmosphere. At the periods of a woman's life when she is most subject to nervous maladies, this danger should be most carefully guarded against. And what shall we say of the nerves of children and invalids? If the former are hard to rear in cities which create hysterics at eight years of age, some blame must certainly be laid upon the air they breathe and the moral conditions in which they have been educated; but some part of the evil must be attributed to the influence exercised by noise on these little

*A Practical Treatise on Roads, Streets and Pavements. By Maj.-Gen. Q. A. Gillmore. New York: D. Van Nostrand.

beings, in whose organization the cerebral predominance is the most marked feature. As for invalids, quiet is of the first importance, and the noise in the streets is the cruelest stumbling block in the way of recovery."

Dr. A. M'Lane Hamilton, Assistant Sanitary Inspector of the city of New York, in an official report dated October 19, 1874, says, "A quiet and noiseless street pavement would advance the health of the population to a great extent. The sufferer from nervous diseases would find relief from the noise of empty omnibuses and wagons rumbling or rattling on the rough stones, in the event of a removal of this nuisance. In fact there would be many more sanitary benefits resulting from a change that I can here detail."

It is not deemed necessary to enlarge further upon this point. The writings of eminent medical practitioners are full of testimony to the pernicious influence of street noise and din upon the health of the population, particularly upon invalids and persons with sensitive nerves.

The noisome and noxious exhalations emanating from the putrescent matter, such as horse-dung and urine, collected and held in the joints of stone pavements, constitutes another sanitary objection to their use in populous towns. Exceptions to wood may be taken upon the same, and even upon stronger grounds, for the material itself undergoes inevitable, and, sometimes, even early and rapid decay, in the process of which the poisonous gases resulting from vegetable decomposition are thrown off.

The joints of a block pavement, whether of wood or stone, constitute, after enlargement by wear, fully one-third of its area, and under the average care, the surface of filth exposed to evaporation, covers fully three fourths of the entire street. This foul organic matter, composed largely of the urine and excrement of different animals, is retained in the joints, ruts and gutters, where it undergoes putrefactive fermentation in warm damp weather, and becomes the fruitful source of noxious effluvium. In dry weather this street soil, of which horse-dung is a large ingredient, floats in the atmosphere and penetrates the dwellings in the form of unwholesome dust, irritating to the eyes and poisonous to the organs of respiration. Its damage to furniture, though serious, is unimportant in this connection. In the side gutters and underlying soil the foul matter exists in a more concentrated form, the supply being constantly renewed from the crown of the street, and in many districts, from the filthy surface drainage of backways and alleys peopled by the poorer classes. Is it too much to say that under such circumstances, the infant population and especially the children of poor people in large towns, can only be reared under such predispositions to disease, as will constitutionally render them an easy prey to epidemics in maturer years?

The foregoing are some of the leading hygienic objections to pavements laid in blocks, whether of stone, wood or other material. There are others peculiar to wood alone arising from its decay, its natural porosity, and the spongy character conferred upon it by wear and crushing.

"Impregnation of the wood with mineral matters, to preserve it from decay, may diminish these evils, but nothing as yet tried prevents the fibres being separated, and the absorption of dung and putrescent matter by the wood being continued. The condition of absorbing mere moisture is of itself bad, but when the surface absorbs and retains putrescent matter, such as horse-dung and urine, it is highly noxious. The blocks of pavement with this material are separated by concussion, and are thus rendered permeable to the surface moisture. Mr. Sharp, who examined some blocks taken up for re-pavement, states that he found them, perfectly stained and saturated with wet and urine at the lower portions, while the upper portions were dry. Mr. Elliott, a member of the society, and for many years a deputy of the Common Council of the city of London, has carefully observed the trials of new modes of pavement there, and objects to the wood that it is continuously wet and damp. Wood is porous; it is composed of bundles of fibres. It absorbs and retains wet, foul wet especially. The fibres of the wood are placed vertically, the upper ends whereof fray out, are abraded and become like painters' brush stumps, and are almost per-

manently dirty, or they break like the handle of a chisel which has been struck with an iron hammer or wooden mallet. This fact is beyond all question. Wood is wet or damp, more or less, except during continued very dry weather. Its structure is admirably adapted to receive and hold, and then give off in evaporation, very foul matters, which taint the atmosphere and so far injure health." (Report of P. Le Neve Foster, Secretary, Society for the encouragement of Arts, Manufacture and Commerce: London, 1873.)

Physicians assert that hospital gangrene frequently results from washing the wooden floors of the wards with water, and that on shipboard, new or moist timber, between decks, impairs the health of the sailors. Fatal epidemics at sea have been traced to timber that has become saturated with putrescent matter, or wet with bilge water.

Prof. Fonssagrives, of Paris, says: "The hygienist cannot, moreover, look favorably upon a street covering consisting of a porous substance capable of absorbing organic matter, and by its own decomposition giving rise to noxious miasma, which, proceeding from so large a surface, cannot be regarded as insignificant. I am convinced that a city with a damp climate, paved entirely with wood, would become a city of marsh fevers."

The dust produced by the abrasion and wear of a wooden pavement is regarded by physicians as extremely irritating to the organs of respiration, and to the eyes, and being light in weight, it floats longer in the atmosphere and is carried to a greater distance, than that from any other material in use for street pavements.

The evidence, from a sanitarian point of view, against the use of wood for paving purposes in populous towns, is very strong, but the evils are not developed to the same extent in all localities. Decomposition begins in two or three years in clayey and retentive soils, while it is very considerably retarded and the wood remains habitually dryer and emits less effluvia where the subsoil is sandy and porous.

The most characteristic features and properties of asphalt pavement have been briefly summarized on page—and it is not deemed necessary to repeat or enlarge upon them here. Professor Fonssagrives remarks that, "The absence of dust, the abatement of noise, the omission of joints—permitting a complete impermeability and thus preventing the putrid infection of the subsoil—are among the precious benefits realized by asphalt streets."

Upon hygienic grounds, therefore, asphalt conspicuously stands first, stone second, and wood third, in order of merit.

The correct inference from the foregoing discussion is that no one pavement combines all the qualities most desirable in a street surface. It cannot be sufficiently rough, or sufficiently soft to give the animals a secure foothold, and at the same time possess that smoothness and hardness which is so essential to easy draught. The advantages of open joints and entire freedom from street filth can not exist together, under any reasonably cheap method of cleansing the surface.

A pavement of impermeable blocks, if laid upon a solid foundation, may be constructed and maintained in a water tight condition, by thoroughly caulking the joints with suitable material, leaving the surface sufficiently rough and open to obviate the objection to a continuous monolithic covering, but roughness, combined with the requisite hardness, is incompatible with the freedom from noise attainable with some kinds of acceptable street surface.

In order, therefore, to obtain the best pavement for any given locality a judicious balancing of characteristic merits is generally necessary. The best pavement, so far as we now know, for all the busiest streets of a populous city, where the traffic is dense, heavy and crowded, is one of rectangular, blocks set on a foundation as good as concrete, or as rubble stone filled in with concrete; and the next best is one of Belgian blocks set in the same manner.

The best pavement for streets of ample width, upon which the daily traffic is not crowded, or for streets largely devoted to light traffic or pleasure-driving or lined on either side with residences, is continuous asphalt for all grades not steeper than 1 in 48 or 50.

If the blocks of compressed asphalt

fulfill their present promise, they may be able to replace those of stone upon streets where the latter are now preferable to a sheet of asphalt on account of the steepness of the grade.

It has been urged, as an objection to a concrete foundation, that it is difficult to take up in order to reach the gas and water pipes. This is true only in the sense that good work is not easily taken to pieces. But such a foundation when torn up or deranged from any cause, can readily be restored to its former condition, and the pavement relaid upon it with all its original smoothness, firmness, and stability, conditions which do not obtain with any kind of pavement laid upon a bed of sand or gravel.

Further extracts from this very practical work we reserve for a future time.

REPORTS OF ENGINEERING SOCIETIES.

At the November meeting of the Boston Society of Civil Engineers, a report was made by a Committee, appointed to consider the introduction of the metric system, and what action the Society ought to take respecting it, and the following resolves and orders were adopted:

Resolved, That it is very desirable that the metric system of weights and measures should be generally adopted, and the irregular standards, now in common use, abandoned.

That the Committee on the metric system be hereafter a standing Committee.

That the Standing Committee prepare a memorial to the Congress of the United States on behalf of this Society, praying that honorable body to enact that, after some fixed date, the metric standards in the office of Weights and Measures, at Washington, shall be the sole authorized public standards.

That approximate weights and measures be procured for the Society rooms.

That the State Board of Education and the Boston School Committee be addressed, urging that the instruction be made more thorough by putting the real weights and measures into the hands of the pupils.

That the Committee be instructed to communicate to the U. S. Commission for testing iron and steel, a request on behalf of this Society that their report may be made in terms of the metric as well as the more common standards.

That the Committee be authorized to open correspondence with other Societies with a view to securing united action in petitioning Congress.

That the Secretary be instructed to publish what action the Society has taken on this matter in the principal newspapers and professional periodicals.

Mr. Edward Appleton read an interesting essay on "The Relations of the Railroads to the People, and to the Government of the State and the Nation, what they are and what they should be." Mr. Appleton began with a description of the erroneous ideas of the community when the first charters were granted, and of the systems and rights of the corporation as now constituted. The question of State management of railroads was discussed, and numerous instances of railroads usurping their authority were cited.

Franklin Institute.—At the December meeting, report from the Committee on Science and Arts, in relation to high pressure steam allowed by law on Western Rivers, embodying the following communication, to be signed by the officers and members of the Institute, and sent to both houses of Congress, was read, and on motion of Mr. Orr was adopted:

"To the Honorable, the Senate and House of Representatives of the United States:

"The undersigned, members of the Franklin Institute of the State of Pennsylvania, for the promotion of the Mechanic Arts, held at Philadelphia, in the State of Pennsylvania, respectfully represent that their attention has been directed to an amendment of an Act of Congress, entitled "an Act to provide for the better security of life on vessels propelled in whole or in part by steam," which amendment was approved December 17th, 1872, and which amended Act permits the use of an increased steam pressure in steam boilers of a certain class.

"By their Committee of Science and the Arts, they have examined the subject, and are of the opinion that the said increase of steam pressure allowed by the said amended Act, is not only improper, but prejudicial to the purpose for which the Act was originally enacted.

"At the request of a former Secretary of the Treasury, this Institute made a series of experiments for the purpose of determining the limits of safety, and means of preventing accidents in steam boilers, and made reports and suggestions upon those subjects, with a draft of a bill for the purposes of protection against accidents from steam boilers, which reports were accepted by the Secretary of the Treasury, and suggestions incorporated in various Acts approved by Congress.

"The said reports are likewise referred to as good authority, upon the subject of the strength of materials to be used for steam boilers, both in this country and in Europe, and the results and opinion expressed have been confirmed by the experience of British and French Scientists, and Government Commissions. We find that the increased pressure permitted by the amended Act of December 17th, 1872, will in many instances prove productive of accident, as permitting the use of steam pressures beyond the limits of safety, and we therefore respectfully recommend that so much of the Act referred to as permits of this increase of steam pressure be rescinded, and that a re-enactment be made, which shall restore the law as it stood before it was amended."

IRON AND STEEL NOTES.

THE LIMIT OF THE CARBURATION OF IRON.—It is impossible to derive a limit of carburation from the variable proportions of carbon entering into commercial iron, combined as it is with manganese, sulphur, etc. To discover whether iron and carbon form a fixed compound, no dependence can be had except on experiments made with compounds containing only iron and carbon, both in a state of purity. Dr. PERCY gives 4.4 per cent. as the maximum of carbon able to combine with iron. KARSTEN gave 5.1 per cent., stating that with this proportion a fixed compound was formed, represented by Fe_4 C. The author could trace no decided difference in combined carbon between white and gray cast iron. The maximum he asserts to be 4.06 per cent., and that any surplus of carbon is in the form of graphite. If an iron containing this quantity solidifies quickly, it is homogeneous, and retains in the solid state the whole of the carbon, whereas, if cooled slowly, it is not homogeneous, there being combined and free carbon, and probably pure iron. Experiments were made at the works of Mr HOLTZER, at Unieux, with Swedish iron containing 0.9961 of metal, of which 22 lb. broken in pieces, with the intervals filled with wood charcoal, were melted in a crucible in a Siemens furnace. The metal was run on a cast iron plate; the lower portion of the mass was white, the upper being gray. The following was the result of the analysis:

	Iron.	Combined Carbon.	Graphite.	Total Carbon.
In the mass.....	95.90	2.10	2.00	4.10
In the white zone.	95.99	3.585	0.425	4.01
In the gray zone..	95.22	2.67	2.11	4.78

The white zone, in which the proportion of graphite was only 0.004, has nearly the theoretical composition Fe^6C, in which

Iron.....	=	94.90
Carbon.....	=	4.10
		100.00

The whole of the carbon was, undoubtedly, in combination in the liquid state, and the graphite was formed on the metal cooling. The author, finally, considers there is nothing more curious than the changes in the nature of iron combined with a maximum of carbon produced by the influence of temperature; gray iron transformed into white, by the union of free carbon and free iron; and, reciprocally, white iron changed into gray, by carbon and iron being set at liberty.

RAILWAY NOTES.

CONTINUOUS FREIGHT TRAFFIC OVER LINES OF DIFFERENT GAUGES.

Editor Van Nostrand's Engineering Magazine:

SIR—I desire to call your attention to the enclosed slip, which you will see is an extract from your magazine:

"A correspondent, Mr. Joseph P. Noyes, of Binghamton, writes us on the above subject as follows: All, or nearly all freight cars have a stout timber or plank crossing under the body of the car, over the centre of each truck, and projecting an inch or two beyond each side of the car, thus forming four points upon which a loaded car may rest without injury, independent of the trucks.

"Upon these same planks are placed three castings, namely, the pivot and two side bearings.

"What I propose is to have a lifting apparatus, with most of its working parts in a pit under the track, to lift a loaded car bodily off the trucks by the four points above mentioned, run out the trucks of one gauge, and run in trucks of another gauge, and let the car down upon them. The pivots, side bearings, and break chains would have to be of uniform pattern; but in other respects all the existing variety of cars and trucks could be used, and every road would keep its *running gear upon its own road.*

"A 10-horse engine would furnish power to transfer several hundred cars in a day, with switch engines to move them.— *Van Nostrand's Engineering Magazine, March*, 1871."

The precise plan has since been very extensively adopted, but was, so far as I know, original with me, and I presume at that time new to you.

I have never sought any pecuniary reward, but would be glad to have the credit of first making the suggestion, if it belongs to me.

Yours Respectfully,
JOSEPH P. NOYES.

Binghamton, Nov. 25th, 1875.

THE SOUDAN RAILWAY.—A project which has long dazzled Egypt by its grandeur—the Soudan Railway—is, writes the correspondent of the *Times* at Alexandria, now being taken seriously in hand. The surveys were made long ago; rolling stock, machinery, and other material have already gone forward in large quantities to the interior, and further shipments are constantly arriving at Alexandria. It is a great undertaking, and must ultimately do good service in the development of the interior, but whether the cost, which is estimated at four millions sterling, is justified by the present state of Egyptian finances, is quite another question, and one worth examination. After leaving the first cataract, the traveler, in ascending the Nile, finds that the river makes a great curve to the west. The stream is shut in by rock, and the country through which it passes is chiefly barren desert. It runs like a silver thread through a dreary waste, only broken by an occasional space where are a few huts, a palm grove, and a patch of cultivation. The bed of the river is often rocky, and above the second cataract navigation is very difficult. This great curve is followed by another in the opposite direction, through country of much the same character. The projected railway is for the purpose of abridging these two curves and bringing Egypt into easy communication with the richer provinces that lie beyond them, while the "rivers of Ethiopia" still enrich what was of old the kingdom of the Queen of Sheba. The railway's whole length is to be 550 miles. It starts on the right bank of the

Nile at Wady Halfa, a scene of dreary desolation, where is the second cataract. The line, as projected, soon leaves the river and goes through desert, over rocky mountains, across wild gorges, down which, after the rainy season, rush tropical flood waters with almost irresistible violence, to rejoin the river at Kohé after a run of 150 miles. At Kohé a bridge, approached by an embankment, is to be erected. The portion of the bridge crossing the deep water channel will consist of one central opening of 80 metres, and two side openings of 30 metres, while the remainder of its length will comprise 26 openings of 18 metres each. The iron work of this bridge, like all the rest of the material of the railway, has to go from Alexandria, 300 miles by rail and 600 miles by river. From Kohé to Ambukol, a village at the southern point of the first curve of the river, there is a run of 200 miles through country of much the same character, passing Berber and several smaller towns, the termini on the river of the great caravan routes from Kordofan and Darfur. From Ambukol there is a final run of 150 miles to Shendy, across the Bahinda Desert, uninhabited save by wandering Bedawee tribes, subject at one season to violent dust storms, and at another covered with vast pools, the reservoirs of the tropical rainfall on the impervious slopes of the granite hills. Shendy, a town on the southern edge of the second great curve of the river, is to be the present terminus of the railway, and is chosen as being the converging point of the various camel routes for the ports of the Red Sea, for Khartoum and the White Nile district, and for Aboo Kharras and the Blue Nile. Apart from our interest in the construction of this railway, on the ground of its absorption of Egyptian resources (adds the correspondent) it may be important to England for an entirely different reason. It is part of a great plan for an extension of the Egyptian railway sytem, so as to connect the Mediterranean with the southern end of the Red Sea. From Alexandria to Cairo, from Cairo to Rhoda, on the Nile, in Upper Egypt, there is already railway communication; and from Rhoda steamers run on the Nile up to the first cataract. The difficulties the cataract presents to the transit of goods are obviated by a railway of five miles already constructed to the upper level of the river. Hence to Wady Halfa there is again communication by steamer on the Nile, though no regular line of boats is as yet established. When this Soudan railway is completed from Wady to Shendy, the Egyptian Government proposes to continue to Massowah, on the Red Sea. But they are prudent enough to wait until the resources of the country will allow of the outlay required for a railway of about the same length as the one now to be constructed. It is also proposed in the future to complete the railway from Cairo to Assouan and Wady Halfa. Supposing these schemes are ever carried out, there would be direct communication between Alexandria and Massowah, and England would have an alternative route to India for her soldiers. She would avoid the dangers of the Red Sea, and curtail the length of the journey to India by three days. The distance between the various points are about as follows:—Alexandria to Cairo, 130 miles complete rail; Cairo to Rhoda, 180 miles complete rail; Rhoda to Thebes, 268 miles steamer; Thebes to Assouan, 133 miles steamer; Assouan to Philæ, 5 miles complete rail; Philæ to Wady Halfa, 210 miles steamer; Wady Halfa to Shendy, 555 proposed rail; Shendy to Massowah, 500 miles (about).—*Architect.*

ENGINEERING STRUCTURES.

REMARKS ON DEEP BORING AND DEEP MINES.—At a recent meeting of the Geological Society of Glasgow, Mr. D. C. Glen, C. E., submitted a paper on this subject. A series of cores was recently presented to the Society by Mr. H. R. Robson, the President of the Institution of Engineers and Shipbuilders in Scotland, and this led to the preparation of the paper, which was illustrated by maps, sections, tools, &c. The author first spoke of the various modes and machines used in what miners call "boring." The object for making a bore-hole into the crust of the earth is to ascertain where and at what depth the various rocks, minerals, and other substances lie; and the bore is sometimes made for the purpose of getting water or petroleum oil. The method of doing this when the ground is soft through which the bore passes is to use what carpenters call a shell auger, with a cross handle fixed on the upper end, and worked by a sufficient number of men. As the hole gets deeper other lengths of rods are screwed on until the required depth is accomplished. Of course during this process the boring rods and auger have to be withdrawn in order to clear out the recess which is filled with the debris; and so a record is also got of the various formations through which the bore has passed. When rock or other hard substances have to be passed through, a steel chisel-pointed jumper is used, alternately lifted and dropped by the men. In this case the debris has to be taken out of the hole by means of a tube provided with a valve at the lower end; and as the jumper is worked in water the debris is in the form of mud, and as some portions of the upper strata fall into the bore-hole, this mud is not always a correct indication of the substance at the bottom of the bore. The upper part of the bore is often lined with tubing, to keep out water and the softer substances, such as sand and mud, which may have been passed through. When the bore gets deep, and the boring rods too heavy to be lifted by four men, a long tree trunk is used as a lever; and sometimes a steam-engine is required to give the desired percussive or jumping action to the boring rods. In order to save the time lost in withdrawing the rods, the Chinese have a system of using rope instead of rigid rods. But this is objectionable, as it sometimes makes the hole untrue, and should the rope break the heavy chisel is left in the hole. Messrs. Mather & Platt, of Manchester, have made some remarkable borings by means of their ingenious cutter, worked by a flat wire-rope.

An improved method of boring is to have, instead of a chisel jumper, a tube with steel

cutters forming a kind of saw, which, being made to revolve, cuts out a solid core or cylinder of the rock passed through. The cylinder, when brought to the surface, not only gives a true sample of the material, but also indicates the dip or inclination of the strata. This system has been improved upon by Major Beaumont, R. E., whose plan is to fix on the bottom of the boring rod a steel tube, which is faced on the lower end with a number of rough uncrystallized diamonds, named carbonite. Unlike the others described, this boring tool is made to revolve with its face in constant contact with the rock, similar to a drill or cutters in boring iron or other metals. A jet of water is forced down the centre of the hollow boring rods, which keeps the face of the cutters cool, and at the same time carries the debris up to the surface. This drill can be worked at a speed of 250 revolutions per minute, at a pressure of from 400 lbs. to 800 lbs. per square inch. It will then bore granite and the hardest limestones at the rate of 2 in. to 3 in. per minute, sandstone at 4 in., and quartz at 1 in. per minute. The diamond known as carbonite have of late been much used for the cutting or dressing of millstones of French "burr." This is accomplished by having a diamond fixed in a small steel holder, which is worked in a straight slide fixed over the face of the stone. By means of the diamonds, the face of the stone is cut or scratched, so as to make a suitable cutting surface for grinding the flour. Latterly carbonite has been applied to the dressing of freestone ashlar by fixing a number of diamonds in a gun-metal or steel block, and giving them a reciprocating and traversing motion over the face of the stone. It is expected that when this machine is completed it will dress from 600 to 1000 square feet per day, or as much as 100 or 150 men can do in the same time.

Mr. Glen gave following examples of the deep borings and sinkings :—The depth of the Artesian well at Grenelle, near Paris, is 1798 ft., and the bore passes into the gault formation. It yields 476 gallons of water per minute, the water rising to height of 32 ft. above the surface, and the temperature is 81¼° Fahr. The well in Trafalgar Square, London, is 393 ft. deep. It descends into the upper chalk. A bore-hole for exploring the coal measures at Creusot, in France, by Herr Kind, is 920 metres, or 3020 English feet, in depth. The deepest bore yet made is in Prussia, at Sperenberg, 23 miles south of Berlin. It has reached the great depth of 4172 ft., and cost £8717, or about 43s. per foot. The deepest coal pit in Scotland is the Victoria Pit, at Nitshill, where the Huslet coal is worked at a depth of 175 fms., or 1050 ft. Monkwearmouth Pit, near Sunderland, was for many years the deepest in England. It is 300 fathoms, or 1800 ft. in depth. Another pit, lying more towards the dip of the coal field, is a few fathoms deeper. The deepest pit in England is now at Dukinfield, near Manchester, belonging to Mr. Astley. Its depth is 408 fms. or 2448 ft. It passes through 22 workable seams of coal. The deepest pits in the world are now in Belgium, in the coal fields lying between Mons, Charleroi, Namur, and Liége. The shafts in several cases are over 750 metres in depth, or 2460 ft.; and one shaft at Gilly, near Charleroi, is 1040 metres deep, or 3411 ft.; and one part has now reached the depth of 3489 ft.—*London Mining Journal.*

ORDNANCE AND NAVAL.

VOORHEES'S STEERING APPARATUS.—The object of this invention is to move a ship's rudder easily and quickly, and to have it under as complete control as volition itself. In order to accomplish this object, this invention utilizes both hand and hydraulic or pneumatic power, and also that of steam.

The said invention consists of a winding mechanism and a hydraulic or pneumatic engine operated by said mechanism, or by an ordinary hand steering wheel and ropes, or by both. The winding mechanism itself is operated by power transmitted from a revolving shaft by means of belts, or by the frictional contact of a pinion on said shaft with the pulleys or gear wheels, which form part of said mechanism.

The operation of this winding mechanism is directly controlled by an auxiliary hand steering wheel, and also by the main steering wheel, through the intervention of the hydraulic engine.

The main steering wheel may be used alone to actuate the rudder, as well when connected either to the winding mechanism or to the hydraulic engine, or to both of them, as when disconnected therefrom, and the auxiliary steering wheel may be used in conjunction with either the main wheel, the winding mechanism, the hydraulic engine, or all of them, as is hereinafter fully described.

Beyond providing for the prevention of shocks to the mechanism and helmsman from the violent action of heavy seas upon the rudder by the use of the hydraulic engine above mentioned, the connections of the several moving parts of this machine consist only of ordinary flexible hide or other rope, and chains and rods where desired, so that, whatever shocks may occur, there can be no rigid connections to be broken, jammed, or worn, such as are usually found in steam and other toothgeared steering mechanisms.

EGERTON'S STEAM FERRY.—Experiments were made with a model of this steam ferry on the lake at the Welsh Harp, Hendon. Mr. Egerton's plan consists in using what may be termed an immense floating platform, propeled by paddlewheels. The structure is composed of three main longitudinal iron cylinders, each 600 feet in length by 26 feet in diameter at the centre, tapering off to a point at each end. The cylinders are divided up transversely into a number of water tight compartments, each properly staged. The cylinders are placed 78 feet apart, and are connected on the tog by means of a series of wrought iron girders, 20 feet deep, thus presenting a platform or ferry 600 feet long by 234 feet wide On the level of the lower flanges of the main girders will be a deck carried on cross girders, the upper deck being carried on the top flanges. The

space between decks will be appropriated to saloons, cabins, engine rooms and other necessary offices, while the upper deck will be appropriated to railway trains, which are to be transported bodily across the Channel from the various railways on either side. The diameters of the cylinders is calculated to be exactly double the height of the highest Channel wave, so that it is expected that no wave will touch the under side of the lower deck. The ends of the tubes are made to taper so as to cleave the water readily and to act as breakwaters to diagonal waves. The whole arrangement will be boxed in between the two decks, and will be made as snug and as comfortable as possible. On each tube, central of its length, will be placed the engines, with a pair of paddlewheels to each engine, steam being taken from two boilers, one being placed in each space intermediately between the tubes. Arrangements are, of course, made for steering the ferry. The model with which the experiments were made at Hendon was 12 feet long by 6 feet wide, and was fitted with boilers and engines arranged as described. The steaming qualities and floating power of the model were demonstrated directly it was placed upon the lake. Unfortunately, however, the experiments had not proceeded far when one of the boilers gave out, and the experiments were brought to a termination. Mr. Egerton's system will, of course, require special landing-places on either side of the Channel, with arrangements for shipping and unshipping the trains. Another proposition of Mr. Egerton's is to construct vessels upon his principle for the transport of cattle from across the Atlantic. For that purpose the central tube would be dispensed with, and several other modifications would be introduced. No doubt Mr. Egerton would succeed in obviating sea-sickness on the Channel by his scheme, although it is one which must involve considerable expense in the new works it will necessitate.—*Iron.*

BOOK NOTICES.

ELEMENTS DE GEOMETRIE PRATIQUE. Par E. A. TARNIER. Paris: Ganthier-Villars. For sale by D. Van Nostrand. Price $4.00.

All the practical applications of ordinary geometry are either given or suggested by this work. There are 340 pages of closely printed text, and a large finely printed atlas. The author presents the applications in the order of the geometrical text books, and gives numerous illustrations of every principle.

The descriptions of the instruments to be used, and the manner of using them, is very full. The applications are extended to the curves employed in architecture.

An English translation would be an invaluable aid to architectural or mechanical students working without an instructor.

EASY RULES FOR THE MEASUREMENT OF EARTHWORKS BY MEANS OF THE PRISMOIDAL FORMULA. By ELLWOOD MORRIS, C. E. Philadelphia. New York: D. Van Nostrand. Price $1.50.

Probably no one subject relating to practice of engineering has received more attention at the hands of writers than this one of Earthwork Computations.

The difference between a bad method and a good one, certainly represents either a large amount of labor, or else a large error in the work.

In the effort to avoid the drudgery of the lengthy but sure methods, many approximate but inaccurate methods have crept into established use. The use of some of the early tables for railway excavation and embankments led to wide departures from accurate estimates; designed only for preliminary work where relative values may be allowed, they are in use for final estimates by some computers.

There are now several good sets of tables by as many different authors, and upon as many different plans of arrangement. Those most in favor with careful engineers present the method of construction and exhibit the limit of possible error.

The most satisfactory of these, to us, is the work before us. In this book the approximate and accurate methods are compared at length, and by aid of elementary geometric demonstration.

How completely this is done, may be seen by the following abstract of the table of contents:

Chapter I. Preliminary Problems. — The Prismoid; Simpson's Rule; Hutton's Prismoidal Rules; Prismoid adapted to Earthwork by MacNeil; The Prismoid in its simplest form computed by several rules; Adaptation of the Prismoidal Formula to Quadrature Cubature, etc.; Application to three round bodies; Equivalence of some important formulas with that for Prismoid.

Chapter II.—First method of computation by mid-section drawn and calculated for area on the basis of Hutton's rule.

Chapter III.—Second method of computation by heights and widths after Simpson's rule.

Chapter IV.—Third method, by means of roots and squares, as modification of the Prismoidal formula.

Chapter V.—Fourth method, regarding the Prismoid as a prism combined with a wedge or pyramid or both.

Chapter VI.—Prof. Gillespie's four rules and a comparison with foregoing third method.

Chapter VII.—Preliminary or hasty estimates made by Simpson's rule for cubature.

Tables for use.

The above abstract, though only a portion of the table of contents, will serve to indicate the scope of the work, and to demonstrate its value to the engineer, the student, or even to the teacher. For the latter, the book presents many excellent suggestions.

Throughout the book there is an abundance of geometrical diagrams.

PYROLOGY, OR FIRE-CHEMISTRY. By W. A. Ross. London: E. & F. N. Spon. For sale by Van Nostrand. Price $15.00.

It may, perhaps, be questioned whether chemical reactions produced in the dry way, and at temperatures ranging up to full redness,

have been as thoroughly studied, whether for systematic or for analytical purposes, as the changes and decompositions occurring in the wet way at temperatures not ordinarily exceeding the boiling-point of water. We have, to be sure, pyro-chemical operations on the large scale in abundance. But have all the processes of the metallurgist, limited as they are in their objects, and in the number of bodies operated upon, been found capable of scientific explanation in accordance with re-received theories? We have, on the other hand, in blowpipe analysis, a body of methods by which the presence of most inorganic bodies may be detected with no less ease than precision, But without at all undervaluing the results of such men as Berzelius, Plattner, Forbes, and others, we may still ask whether the standard treatises on the use of the blowpipe include every operation by which the presence of elements or of their compounds "can be discovered in the dry way?" This question Major Ross answers in the negative. Some time ago he communicated certain interesting papers to the "Chemical News," in which he made known a number of novelties which promise, at least, to be useful. Whether these new methods have been tested by mineralogists and chemists, and if so with what result, we are unable to say. We cannot lay our hands upon any memoir, English or foreign, in which they are criticised. Among these novelties, real or imaginary, are "the vesiculation of borax with oxydes dissolved in it, and the corresponding crystals, which form on the surface of the vesicle, laid on cotton in ordinarily moist atmosphere; the vesiculation of boric acid containing alkaline traces, and the detection of potash in them by breathing on the vesicle; the violet color given by cobalt oxyde to phosphoric acid, and the means of thus quantitatively estimating alkalies, which turn blue in certain proportions; spherospheres, or contained balls, formed by cobalt oxyde in boric acid beads; metallic-looking films formed over beds of boric and phosphoric acid held in a good hydro-carbonous pyrocone; decoloration of cobalt with soda by arsenic acid; delicate reactions of oxyde of silver in phosphoric acid, by which it can be detected in most galenas; structure of pyrocones; cobalt solution, reaction given by lime; reactions of chlorine and fluorine; curious reaction of soda in pyrophosphate of lime; separation of substances, especially of metals in alloys, by utilizing their different attractions for heat; the use of aluminium plate as a support; quantitative assay of sulphide of copper by oxyde of lead in phosphoric acid; separation of silica, alumina, ceria, and the alkaline earths, including didymia and lanthana, by means of their behavior in boric acid; quantitative determination of chemical water in clear fused boric acid, by means of a magnesian borate balls; separation of silica and alumina by lime borate balls; separation of didymium and lanthanum borates from ceric oxyde; detection of fluorine, chlorine, and sulphur by means of oxyde of copper in a phosphoric acid bed; detection of phosphoric acid in tourmaline by boric acid; new reaction obtained by a solution of manganese in sulphuric acid; mangano-cobalt solution with the same view; artificial zeolite formed by heating a mixture of potash and pure alumina, for the purpose of detecting alumina or lime, or caustic alkali; yellow and brown oxydes of thallium obtained on aluminium plate from the metal; detection of sulphuric acid by the effervescence caused by adding a drop of water to a natural sulphate (as gypsum) which has been fused with soda on aluminium plate; decrepitation observed to be peculiar to crystalline forms; solution and separation of silica by boiling with boric or phosphoric acid dissolved in water; sublimation of gold, silver, and other metals by fusing them in the oxydizing flame with a minute proportion of lead or charcoal over aluminium; the curious crystallization of soda combined with a small proportion of lime; the determination of the mineral constituents of animal or vegetal organisms by burning the latter on a bead of boric acid." It is utterly out of our power to judge these novelties in the only fair manner; that is, by working through them one by one and deciding in how far the author's observations and methods can be actually verified. But this is a task which ought not to be neglected, and we are strongly of opinion that, whatever might be the result, such an undertaking would be well worth the while of any student endowed with the needful leisure, skill, and patience. Even if we suppose, for argument sake which is extremely improbable, that the author should be found mistaken in every instance, the detection of his errors could not fail to be profoundly instructive.

We have no great respect for writers who take up some subject in an unsystematic way, and after a desultory course of reading think themselves entitled to lay down the law and to point out the supposed mistakes of received authorities. Of such men, and of their productions, every page of which testifies to the want of all fundamental intellectual discipline, every scientific critic is absolutely sick. They swarm upon us like a new plague of flies. But Major Ross belongs to a totally different category. He is no "paper-philosopher," but an earnest, careful, persevering worker, possessed of a fruitful and suggestive mind; and his conclusions, therefore, however unexpected, cannot be without value. We strongly recommend his work to the attention of chemists and mineralogists in the belief that they will find it both interesting and useful. —*Quarterly Journal of Science*

CARPENTRY AND JOINERY FOR AMATEURS; containing a full description of the various tools required; with Practical Instruction for their Use. By the Author of "Turning for Amateurs," &c. London: The "Bazaar" Office.

This book contains more, in one sense, than its unpretending title page claims for it; as it gives very extended and full information not only as to the use of tools, but as to the principles and methods to be followed in the principal classes of operations included in joiner's work; perhaps in some instances going beyond

what amateur workers are likely to undertake, though this does not make the book the less useful. The illustrations of tools and of the various ways of putting wood together are very well and clearly executed; and the writing generally is marked by clearness and terseness, without omitting anything essential. The writer looks at his subject from more than a merely practical point of view, as his remarks in regard to veneering show. In giving directions for this process, he comments on the difficulties in regard to fitting veneer to curved surfaces, for which special holdfast or "cauls" have to be made, for keeping the veneer in its place until it binds properly; "and hence it is, among other causes, that we see so many repetitions of the same pattern issued from any particular shop, and such unwillingness to depart from them. There is far more freedom in this respect when veneer is not intended to be used." Outsiders, as he says, are not acquainted with the complication of the process of veneering in case of articles that present any but the simplest forms; and any common sense person reading for the first time even the brief instructions and description of the process given here for the use of amateurs, would probably feel a degree of wonder at the trouble that is taken to get over the difficulties which oppose the practice of a sham method, half the care and labor bestowed upon which would suffice to produce some really good and honest work of a high class, in perfectly good though less costly material than that generally used for veneering. In general, throughout the book, the best way of doing everything, in regard to the use of the material, is well brought out; the exception, as usual, being in the case where some form supposed by force of habit and fashion to be ornamental or artistic is to be illustrated, and then we have such things as the claw leg for a pillar table (p. 121), or the spiral or scroll leg (p. 123); forms not only wretched in themselves as regards appearance, but such as no sensible workman in wood, if not under the influence of the demon "taste," would ever think of adopting. This is always the case, we find, in these practical books, whenever the writers travel out of the line of the practical into what they regard as the ornamental, in which they are governed, generally, by mere habit and bad precedent, without any consideration as to whether the forms portrayed really are consonant with the best way of working the material, or with the purpose for which the object is designed.—*Builder.*

BRITISH MANUFACTURING INDUSTRIES. By G. PHILLIPS BEVAN. London: Stanford. For sale by D. Van Nostrand. Price $1.75 per vol.

When we received the two volumes of this series which are now before us, we naturally turned to the preface to see what could be the object of their publication. The names of the writers on the different subjects are sufficient guarantee that the matter is interesting, but what is the *raison d'etre* of the "series"? Its object is, we are told, to bring into one focus the leading features and present position of the most important industries of the kingdom, so as to enable the general reader to comprehend the enormous development that has taken place within the last twenty or thirty years. The books "do not lay claim to being a technical guide to each industry;" then why were they written? The general reader will glance over them and throw them aside, and it is acknowledged that those interested in the trades described will not derive much knowledge from their perusal. However, that is for the consideration of the publisher and the public. The matter is well written, and so far as we have examined it, is trustworthy. Mr. Arnoux writes on "Pottery," Professor Barff on "Glass Silicates," and Mr. J. H. Pollen on "Furniture and Woodwork." These essays are very well as far as they go, but we doubt their utility. The information is rarely of practical value, while often it is of an indefinite nature, as, for instance, in Professor Barff's essay on so interesting a subject as looking-glasses. He describes the method of covering the sheet of tinfoil with mercury, and then says "the glass plate to be silvered, having been made perfectly clean, is *floated* upon the surface of the quicksilver—an operation requiring care—and is then covered all over with weights," &c. This term "floating" is deceptive, and does not instruct the reader in the actual method, which, as our readers are aware, consists in sliding the piece of glass on to the tinfoil in such a manner that the surface of the mercury is entirely removed—a most essential detail of the mercury process of silvering looking-glasses. It may be said that this is a technicality, but when a writer goes so far into his subject as to describe the process of floating, he might just as well describe it accurately. In another volume we find an essay on "Iron and Steel," by Mattieu Williams, which we need scarcely say is good as far as it was possible for its author to make it so in the space at his disposal. "Copper Smelting," by J. A. Phillips, and "Brass Founding," by W. Graham, are also well treated, but are of little use to the practical worker or the amateur. Indeed, each of the subjects requires a volume to itself, though of course if there is a reading public interested in the perusal of these descriptions of our industries, the *raison d' etre* of the present volumes is established. Certainly the editor has selected his authors well, for D. K. Clarke writes on "Railways," R. Sabine on "Telegraphs," Dr. Rimbault on "Musical Instruments," and Professor W. W. Smyth on "Collieries"—sufficient guarantees that the best that could be done in the space has been done.—*English Mechanic.*

SKETCHES OF ARTISTIC FURNITURE. By COLLINSON & LOCK. London. For sale by Van Nostrand. Price $5.25.

This is just what its title signifies, a collection of artistic designs for furniture. It is a neat folio, and the designs are good. They include the articles of constant use as well as those that are professedly ornamental only.

This book will doubtless find a use with many, to whom a few years since it would have been useless.

Furniture dealers are now prepared for orders from designs, and the custom of thus furnishing houses and apartments seem likely to prevail widely.

DESCRIPTION OF THE INTERNATIONAL BRIDGE OVER THE NIAGARA RIVER, NEAR BUFFALO. Toronto: Copp, Clark & Co. For sale by Van Nostrand. Price $9.00.

This is a fine quarto of 65 pages, and illustrated with twenty-one plates, most of them of large size.

The text gives in separate chapters the history of the enterprise: The Characteristics of the River; The Foundations; The Masonry; The Superstructure; Ice.

The history of the progress of any great engineering work, if it include descriptions in detail of the technical operations, is a valuable addition to professional literature.

The present work seems to be wanting in no respect the qualities that entitle it to such a place in our scientific libraries.

MISCELLANEOUS.

ON THE SINKING OF A PAIR OF IRON SHAFTS FOR AN EXPERIMENTAL AMBER MINE.—The supply of amber in commerce is mainly derived from the district of Samland, near Konisberg, in Eastern Prussia, where it occurs in a deposit locally called "blue earth," in a brown coal formation of the Tertiary age. Until lately amber was got chiefly by dredging and by collecting the fragments thrown up during heavy gales on to the sea-coast, or by shallow, irregular diggings a short distance inland. Recent geological researches having proved the continuity of the amber-bearing beds, the Prussian Government considered it desirable to start an experimental mine, to determine the conditions upon which concessions might be granted to private individuals, the right of working amber being, one of the Crown privileges in Prussia.

The locality selected is at Nortyken, in Samland, where the amber-bearing bed has been found by boring to a depth of 140 feet. The section is as follows:

Hard blue earth, without amber.	Feet 2
Blue earth, rich in amber	4.9
Barren earth, no amber	1

The level of the bed is 18.7 below the level of the Baltic. The overlying strata, 140 ft. thick, consists of sands and clays belonging to the brown coal formation. The works planned for laying out the mine consist of two winding shafts, two bore-holes for pumping, and the necessary engines and boilers. As a large quantity of water was to be looked for in sinking, it was decided to bore the shafts and line them with wrought iron tubes. The depth of both shafts is about 147 ft. and the distance between them is 73.8 ft.

The boring head was a horizontal bar, carrying four chisels for cutting into the bottom of the hole, and two at each end radially for describing the outer curve of the shafts. It weighed about 17 cwt. The boring rods of wrought-iron were of two sizes, one being an inch square, used in boring percussively; and the other 2 inches square, used when a twisting strain was applied.

The sand-pumps, or shells, for removing the detritus produced in the boring, were of two sizes; the larger being 3.1 ft., and the smaller 2.1 ft. in diameter, the length in each case being 6.4 ft. They were wrought iron cylinders with clack valves at the bottom, but the suspension was so arranged that when brought full to the surface they could be emptied by being tipped like a bucket, without the necessity of being detached from the rods. The iron tubes lining the shafts are of best boiler plate, 0.8 in. thick, and 4.6 ft. internal diameter, in lengths of 4 ft., joined by internal rings of the same thickness and riveted. The tube is further strengthened internally by three longitudinal strips of iron of the same thickness. The bottom length of tube is of double thickness, and terminates in a cutting shoe of triangular section. The total weights of the lining tubes are 44 tons for shaft No. 1, and 45.6 tons for shaft no 2, or rather more than 1 ton per lineal yard.

The sinking of the tubes was effected by pressure applied by screws. A cast-iron ring grooved underneath to fit the tube, and having four perforated lugs through which the pressure screws passed, was placed on the top of the tube, and the pressure was applied to the nuts by men working spanners. The lower ends of the screws were attached to a fixed point or abutment formed by a timber platform loaded with cast-iron; four screws were placed at equal distances around the circumference of the tube. The spanners were slung by tackles for convenience of manipulation, and from four to five men worked at each, so that from 16 to 20 men were employed in pressing down the cylinder. The amonnt of material displaced for each length of tube was about 53 cubic feet, which was removed in four or five fillings of the larger-sized shell in about six working hours. The sinking of the tube occupied about four hours, so that one complete length of the shaft tube was sunk and a fresh length slung and adjusted for riveting in each shift of 12 hours. From the sandy nature of the ground little actual boring was required, the use of the chisels being confined to cutting through beds of shale. The work was done in day and night shifts of 12 hours, with an average of 27 men. No. 1 shaft was completed in 121 shifts, and No. 2 in 106 shifts, including both boring and riveting. The latter operation occupied rather more than half the time. The total cost of the two shafts was as follows:

Wrought-iron lining tubes	£4093
Boring Plant	1259
Carriage	720
Labor for riveting and sinking	715—£6787

The water level during the sinking was constant at 32.8 ft. below the surface, the shaft being about 46 ft. above the bottom of the valley.

—H. KUHN, Zeitschrift fur das Berg-Hutten- und Salienenwesen.—H. B., in *Proceedings of London Institution of Civil Engineers.*